Prof. Asa Gray
Harvard University
Cambridge
Boston
U. States

Bromley
APR 26
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P. Paid

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Down Farnborough Kent
April 17th
1855

My dear Sir

I hope that you will remember that I had the pleasure of being introduced to you at Kew. I want to beg a great favour of you, for which I will know I can offer no apology. But the favour will not, I think, cause you much trouble & will greatly oblige me. As I am no Botanist, it will seem so absurd to you by asking botanical questions, that I may premise that I have for several years been collecting facts on "Variation", & when I find that any general remark seems to hold good amongst animals, I try to test it in Plants. — I have the greatest anxiety about the alpine Flora of the U.S., & I have copied out of your Manual the enclosed list; now I want to know whether you will be so very kind as to append from memory (I have not for one instant the presumption to wish you to look to authorities) the other habitats or range of these plants: appending "Arctic" to those such as are confined to the mountains of the U.S. — "Arctic Am." to such as are also found in Arctic America. — "Arctic Eur." to those also found in Arctic Europe: — & "Alpes" to those found in any mountains of Europe. — I have compared the list with the plants of Britain, but I am of course afraid of trusting to myself, from ignorance of synonymy.—

I see that there are 22 species common to the White Mts & the Mts of New York, will you tell me about how wide a space of low land, on which these alpine plants cannot grow, separates these mountains: I can hardly judge from the height not being marked on the representation of the mountains of Vermont.—

I venture to ask for one more piece of information, viz. whether you have

anywhere published a list of the phanerogamic species common to Europe, as has been done with the shells & Birds, so that a non-Botanist may judge a little on the ~~[illegible]~~ relationships of the two floras. Such a list would be of extreme interest for me in several points of view & I should think for others. I suppose there would not be more than a few hundred out of the 2000 species in your memoir. Should you think it very presumptuous in me to suggest to you to publish (if not already done) such a list in some Journal? — I would do it, for myself, but I shd. assuredly fall into many blunders. [illegible] can afford you, that I possess the presumption it is in me, as a Botanist, to make even the most trifling suggestion to such a Botanist as yourself; but from what I have heard of you from our dear & kind friend Hooker, I hope & think that you will forgive me, & believe me, with much respect,

Dear Sir

Your very faithfully

Charles Darwin

Down Farnborough Kent

April 25th

My dear Sir

I hope that you will remember that I had the pleasure of being introduced to you at Kew. I want to beg a great favour of you, for which I well know I can offer no apology. But the favour will not, I think, cause you much trouble & will greatly oblige me. As I am no Botanist, it will seem so absurd to you my asking botanical questions, that I may premise that I have for several years been collecting facts on "Variation", & when I find that any general remark seems to hold good amongst animals, I try to test it in Plants.—

I have the greatest curiosity about the alpine Flora of the U.S., & I have copied out of your Manual the enclosed list; now I want to know whether you will be so very kind as to append from memory (I have not for one instant the presumption to wish you to look to authorities) the other habitats or range of these plants: appending "Indig." for such as are confined to the mountains of the U.S.— "Arctic Am." to such as are also found in Arctic America.— "Arctic Eu." to those also found in Arctic Europe:—& "Alps" to those found on any mountains of Europe.—"& Arct. Asia" I have compared the list with the plants of Britain, but I am of course afraid of trusting to myself, from ignorance of synonyms &c.—

I see that there are 22 species common to the White Mts & the Mts of New York, will you tell me about how wide a space of low land, on which these alpine plants cannot grow, separates these mountains: I can hardly judge from the height not being marked on the prolongation of the mountains of Vermont.—

I venture to ask for one more piece of information, viz. whether you have anywhere published a list of the phanerogamic species common to Europe, as has been done with the shells & Birds, so that a non-Botanist may judge a little on the relationship of the two floras. Such a list would be of extreme interest for me in several points of view & I should think for others. I suppose there would not be more than a few hundred out of the 2004 species in your Manual. Should you think it very presumptuous in me to suggest to you to publish (if not already done) such a list in some Journal?— I would do it for myself, but I shd assuredly fall into many blunders. I can assure you, that I perceive how presumptuous it is in me, not a Botanist, to make even the most trifling suggestion to such a Botanist as yourself; but from what I saw & have heard of you from our dear & kind friend Hooker, I hope & think that you will forgive me, & believe me, with much respect,

Dear Sir
Your's very faithfully
Charles Darwin

of N. America. The range of the plants, to the East & West, viz whether most found in Greenland & Western Europe, or in E. Asia appears to me a very interesting point as tending to show whether the migration has been Eastward or Westward. — Pray believe me, that I am most entirely conscious that the only use of these remarks is to show a Botanist what points a non-Botanist is curious to know; for I think everyone who studies profoundly a subject often becomes unaware what points the ignorant require

Down Farnborough Kent
June 8th
[1855]

My dear Sir

I thank you cordially for your remarkably kind letter of the 22d ult.o, & for the extremely pleasant & flipping manner in which you have taken my rather troublesome questions. I can hardly tell you how much your list of Alpine plants has interested me, & I can now in some degree picture to myself the plants of your alpine summits. The new Edit. of your Manual is capital news for me: I know from

you prefer the perfect you are for
from, but it would take no
place to append (Eu.) in brackets
to every European plant, & as
far as I am concerned this
would answer every purpose. From
my own experience whilst making
out English plants in our herbarium,
it has often struck me, how
much interest it would give
if some notice of their range
had been given, & so I cannot
doubt your American enquirers,
& beginners w^d. much like to
know which of their plants were
indigenous, & which European.
Would it not be well in the
Alpine plants to append the
very same addition which
you have not sent me in
No. 8; though here, owing to
your kindness, I do not speak
selfishly, but merely for bona
Americans publico. — I presume
it w^d. be too troublesome to give
in your summaries the habitats of these plants found
West of Rocky mountains; & likewise
those found in Eastern Asia,
taking the Genesei (?) which, if
I remember right according to
Gmelin is the main partition
line of Siberia. Perhaps Siberia
more concerns the northern Flora

the proportional numbers of the families. — Then I make out from your manuscript that of the indigenous plants the proportion of the Umbelliferæ are $\frac{35}{1798} = \frac{1}{49}$; for without one knows the whole numbers, one cannot judge how really close the numbers of the plants of the same family are in two distant countries; but very likely you may think this superfluous. — Relativing these proportional numbers, I may give as an instance of the rate of





information. I am so very glad that you think of drawing up some notice on geographical distribution, for the area of the manuscript strikes me as in some points better adapted for comparison with Europe than that of the whole of N. America. — You ask me to state definitely some of the points on which I most wish for information; but I really hardly can, for they are so vague, & I rather wish to see what results will come out from comparison,

then have an set defined object. I presume that like other Botanists you would give for your area, the proportions (leaving out introduced plants) to the whole of the great leading families: this in our point I had intended to tabulate from your Book, but of course I could have done it only very imperfectly. I should also, of course have ascertained the proportion to the whole flora of the European plants (leaving out introduced) & of the separate great families, in order to speculate on means of transportal. By the way I ventured to send a few days ago a copy of the Gardeners' Chronicle, with a short report of some trifling experiment which I have been trying on the power of seeds to withstand sea-water. I do not know whether it has struck you, but it has to me, that it would be advisable for Botanists to give in whole numbers, as well as in the lowest fraction,

So make, (according to my ideas perfection perfect) one ought to be told whether there are other cases like Erica of Europe or in ~~the~~ general common in N. and not found in your area. —

But honestly I feel that it is quite ridiculous my writing to you at such length on such subject, but ~~as~~ you have asked me, I do it gratefully, & write to you, just as I should to Hooker, who often laughs at

[illegible] & how vague & futile my data are

points, which attempt to work out, that reflecting on Dr. Brown & Hooker's remark, that nearly identically the proportional number of the great families, & in two, or continents, shows probably that they were once continuous & united, I thought I would calculate the proportions, of, for instance, the introduced compositae in Gt. Britain to all the introduced plants, & the result was $\frac{10}{92} = \frac{1}{9.2}$. In our aboriginal or indigenous flora the proportion is $\frac{1}{10}$; & in many other cases I found an equally striking correspondence: I then took your Manual, &

worked out the same question;
~~here~~ I found in the Compositæ
an almost equally striking
correspondence, viz. $\frac{24}{206} = \frac{1}{8}$ in
the introduced plants, and
$\frac{223}{1798} = \frac{1}{8}$ in the indigenous; but
when I came to the other
~~orders~~ families I found
the proportion entirely different
showing that the coincidences
in the British Flora were
entirely accidental! —

You will, I presume, give
the proportion of the species to
the genera, i.e. show on an
average how many species each
genus contains; though I have
done this for myself. —
If it would not be too troublesome
do you not think it will be
very interesting, & give a very
good idea of your Flora, to
divide the species into 3
groups, viz. (a) species common to
the Old World, stretching over Europe
& Asia (b) indigenous species,
but belonging to genera found
in the Old World, & (C) species
belonging to genera confined
to America or the New World.

that you thought specifically, but
which you could conceive the some
other good Botanist might
think of a race or variety; —
or again a species that you
had truth, though having
opportunities of ~~judging it~~ knowing it rarely,
discriminating from some other species.
Supposing that you were inclined
to be so very kind as to
do this, & could (which I
do not expect) spare the time,
as I have said, a mere
copy to each such species
in any useful proof-sheets,

Page 2.

me unspecifically, & I am sure
you have ~~good~~ better reason to
do so. —

There is one point, on which I am
most anxious for information; &
I mention it with the greatest
hesitation, & only in the full
belief that you will believe
me that I have not the
folly & presumption to hope for
a second that you will give it,
without you can ~~do it~~ with my
little trouble. The point can only
present interest to one best
myself, which makes the case totally

different from geographical distribution.
The only way in which, I think,
you possibly could do it with
little trouble, wd be to bear in
mind, whilst correcting for Prof.
Hooker of the manuscript, my question,
& put a cross or mark to
the species, & whenever sending
a pencil to Hooker to let
me have such old sheets.
But this wd give you the
trouble of remembering my
question, & I can hardly
hope or expect that you will
do it. — But I will just
mention what I want, it is,
to have marked the "close
species" in a flora, so as
to compare in different
floras whether the same genera
have "close species," & for
other purposes too vague to
enumerate. — I have attempted
by Hooker's help to ascertain
in a similar way whether
the different species of
the same genera in Britain
are a quarter of the flora &
varieties or present varieties.
The definition I should give
of a "close species" was one

Yes, I

would give me the information desired, which, I may add, I know must be vague. –

How can I apologise enough for all my presumption, & the extreme length of this letter? The great-goodnature of your letter to me, has been partly the cause, so that, as it is too often the case in this world, you are punished for your good deeds.

With hearty thanks

Believe me

Yours very truly & gratefully

Ch. Darwin

over

P.S. Thank you for answering my questions about the distances of the Alpine & Anti-Alpine lower land — remarkably: I have no very good map, but I cannot make what you say tally at all, & suspect that you have written wrong figures. You say that the White Hills are separated from the Green mountains (respectively?) by 150 or 200 miles of intervening lower country; & the Green Hills from those of New York by about the same distance, — which wd. make 300 – 400 miles — Perhaps at some future time, you would tell me how this can be. —

Some of my immersed seeds have come up after 82 & 85 days immersion, viz Radishes, Beet, Asparagus, Capsicum, Oats, Cucumber, Rhubarb, Lettuce, Carrots, Celery, & Onions. —



Down Farnborough Kent
July 21st

My dear Dr. Gray

I must write one line to thank you for your letter of the 30th ult., & for the extremely kind manner in which you have acceded to my request (which I assure you I long hesitated whether I would make) about marking the chosen species. I do not quite understand from Hooker's note, whether the sheets have arrived, but I fancy not; but I shall hear again in two or three days. Hooker read over your letter, & seems

to have been as much struck with it as I have been, — as showing in a most striking manner the geographical affinities of species, & the difficulty of ascertaining what are species. — Your discussion on connecting & separating forms seems to me so philosophical. The I must hope that some day you will be as good as your word & write an "Essay on Species".

I hope, also, before end of year to hear the you have found time to write on the geographical distribution of the U. S. plants; & if my letter caused you to do this some year or two before you otherwise would have done it, I shall congratulate myself in private, at having done good Botanical work.

Pray believe me with cordial thanks

Your very sincerely

Charles Darwin

Mr. H. C. Watson has marked for me
the British Flora, & the same result
is given. I know how vague all
such results must be & these
may be some fallacy in the
result, but I cannot detect
it; & am inclined to believe
that the above proposition may
be trusted; but I shall of
course try to test it by
other means. — Pray accept
my thanks & cordial thanks for
all your very great kindness,
& believe me. Your very truly
Ch. Darwin

Have you seen Decandolle's new
great work on Geographical Botany.
Hooker thinks very well
of it. —

[should the fallacy be apparent to you, I shd be most grateful to be informed]

Down Farnborough Kent
Aug. 24th

My dear Dr. Gray
I received your letter about a fortnight
ago & immediately forwarded the
note to Hooker, who since then has
started for a tour in Germany with
Dr. Priestley's eldest son. —
I am sorry you shd have had such
trouble about the Dyticus, but it
was not my causing.
And now I really hardly know how
to thank you enough for the very
great trouble which the list of
"close species" must have caused
you. — What knowledge & labour &
judgment is condensed in that
little sheet of note-paper! I fear
the you will think the Biscay note
at all worth the labour; but I

can only say that if I could have done it myself, I would have done it, had it caused me ten times the labour which it must have caused you.

I had met with a remark by Fries that the species of large genera are more closely related to each other, than are the species of small genera. I consulted a very good entomologist, & Hooker & Bentham, & they did not at all believe in this. But several facts & considerations, nevertheless, made me think that there might be some truth in it; and all general statements of such kind are my object in my eclectic, pedling sort of work to test. It occurred to me that if I could get some good systematists, not species-splitters, to mark (without the object being known) the close species in a list; then if I counted the average number of the species in such genera, & compared it with the general average [for this end all the genera with single species have to be omitted; & I have omitted Salix & Carex also] of the species to the genera in the same country; it would, to a certain extent, tell whether on average the close species occurred in the larger genera. Now in your No. 8 list (Salix & Carex being omitted) there are 115 genera & there have 6.37 species to genus: whereas in your Manual (omitting Salix & Carex & all genera with single species) the average is 4.67. So that it seems that when many ~~sp~~ organic forms are allied, making what is called a genus, some of them are apt to be more closely allied than are the species ~~other~~ in the smaller genera.

P.S. May 10

As it is ten to one you will not be able to give me any facts on the following head, I venture to put it before you for the chance. —

I have met several (chiefly amongst animals) cases, so many that I can hardly think it purely accidental, in which, when ~~one species a~~ ~~character~~ the species of a genus differed in some organ or part, which is usually constant in the species of the same genus, then that one or more of the species individually varied in some degree in this same organ or character. Now to give a very bad example, I

remember seeing it remarked
expected that the position of
the embryo differed in a very
unusual manner in the species
of Helianthemum; & hence,
if there be any truth in
the above rule, some of the
species ~~ought~~ might be
expected to vary in this
same respect. —

9. Give good example in animal kingdom
in Pytgoma (a genus) the generalen values
(usually very constant) differ wonderfully in the
different species, & in some of the
species, these values, (usually so constant)
vary in the most perplexing manner,
10. that they differ even in the same
species, than often in different species
of different genera —

[Excuse the poor paper I am in a shed]

Down Bromley Kent
May 2.

My dear Dr. Gray

I have received your very kind note of Apr. 8th. In truth it is preposterous in me to give you hints; but it will give me real pleasure to write to you just as I talk to Hooker, who says my questions are sometimes suggestive owing to my comparing the ranges in different kingdoms of nature. I will make no further apologies about my presumption; but will just tell you (though I am certain there will be very little new in what I suggest & ask) the points on which I am very anxious to hear about. — I forget whether you include Arctic America, but if so for comparison with other parts of world c. I would exclude the

Arctic & Alpine or Arctic, as belonging to a quite distinct category. When excluding the naturalized, I think Decandolle must be right in dividing the exclusive, giving list, of plants & claiming found in cultivated land, even when it is not known that they have been introduced by man. — I would give list, ~~[illegible]~~ of temperate plants (if any) found in Eastern Asia, China & Japan & not elsewhere. — Nothing would give me a better idea of Flora of U. S. than the proportion of the genera to all the genera, which are confined to America; & the proportion of genera, confined to America & Eastern Asia with Japan; the remaining genera will be common to America & Europe & rest of world; I presume it will be impossible to show any especial affinity in genera, (if even so few) between America & Western Europe. America might be related to Eastern Asia (although excluding Arctic forms) by a genus having the same species confined

ti then two regions; or it might be related by the genus having different species, the genus itself not ~~having species~~ being found elsewhere. The relation of the genera (~~is not such~~ (excluding identical species) seems to me a most important element in geographical distribution often ignored, & I propose of more difficult application in plants than in animals, owing to the wider ranges of plants; but I find in N. Zealand (from Hooker) the the consideration of genera with representative species tells the story, even plainer than the identity of the species with different parts of the world. — I should like to see the genera of the U. States, say 500 (excluding arctic & alpine) divided into 3 classes, with the proportions given, thus,

$\frac{100}{500}$ american genera

$\frac{200}{500}$ Old world genera, but not having any identical species in common. —

$\frac{200}{500}$ Old world genera, but having some identical species in common: supposing the

then 200 genera included 600 U.S. plants;
then the 600 would be the ~~[illegible]~~
to the fraction ~~[illegible]~~ of the species
in common to the Old World. — But
I am running on at a foolish
length. —

There is an interesting discussion in Decandolle
(about p. ~~[illegible]~~ 503 to 514) on the relation of the size
of Families to the average range of the
individual species; I cannot but think
from some facts which I collected long before
De Candolle appeared, that he is on wrong
scent in having taken Families (owing
to their including too great a diversity
in the constitution of the species), but
that if he had taken Genera, he
would have found that the individual
species in large genera range over

a greater area than do the species in small genera: I think if you have materials that this would be well worth working out; for it is a very singular relation. —

With respect to naturalised plants; are any social with you, which are not so in their parent country? I am surprised that the importance of this, has not been struck De Candolle. Of these naturalised plants are any or many very variable in your opinion, than the average of your U. S. plants: I am aware how very vague this must be: but De Candolle has stated that the naturalised plants do not present varieties; but being very variable & presenting distinct varieties

seems to me rather a different case: if you would kindly take the trouble to answer this question, I shd be very much obliged, whether or no, you will enter on such points in your Essay. —

With respect to such plants, which have their southern limits within your area, are the individuals ever or often stunted in their growth or unhealthy: I have in vain endeavoured to find any Botanist who has observed this point; but I have seen some remarks of Bentham on the trees in the S. Texan seem in this respect to behave rather differently from other plants. —

It would be a very curious point, but I fear you must think it out of your Essay, to compare the list of European plants in Tierra del Fuego (in Hooker) with those in N. America; for without multiple creation, I think we must admit the all, or nearly all, in T. del Fuego, must have travelled through ~~your country~~ N. America, & so far they do concern you. —

The discussion on social plants (vague as the terms & facts are) ~~strike~~ is decidedly striking me as to best, which I have ever seen: two points strike me as eminently remarkable in them: that they should ever

be social close to their extreme limits; & secondly that species having an extremely confined range, get sh? be social where they do occur: I sh? be infinitely pleased either if letter or publishing for any cases & on these heads, more especially in regard to a species remaining or ceasing to be social, on the confines of its range.

There is one other point, on which I individually sh? be extremely much pleased, if you could spare the time & trouble in little bit & inform me: viz whether there are any cases of the same species being more variable in U. S. than in other countries (or in different parts of U. S. in which it is found)?

Wahlenberg says generally that the same species in Grey S. becomes more variable

this is extreme rarity. Even this man
am I anxious to know whether any of
the genera, which have most of these
species horribly variable (as Rubus or
Hieracium or) in Europe, or other part of
world, are less variable in the States:
or the reverse case, whether you have
any ordinary genera with you, which
are less ordinary in other countries.
Any information on this head, would be
a real kindness to me. —

I suppose you there is too great; but a
simple list in <u>close column</u> in <u>small type</u> of all
the species, genera, & forms, each consecutively
numbered, has always struck me as
most useful; & Hooker repeatedly the has
did not give such list, in his New Zealand &
other Floras. — I am sure I have

Gray 4

Given you a larger dose of questions than you bargained for, & I have kept my word & treated you just as I do others. Nevertheless if anything occurs to me during the next two months, I will write freely, believing that you will forgive me & not think me very presumptuous.

How well DeCandolle shows the necessity of conferring nearly equal areas for propagation of families!

My dear Dr. Gray

Yours very sincerely

Charles Darwin

I have retained this letter & it really is not worth sending, except for my own sake: I see I forgot in beginning to state that it appeared to me that the b. heads of your & my ~~too~~ included almost every point which could be desired, & therefore that I had little to say —

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Aug 1856

Down Bromley Kent

Aug 24

My dear Dr. Gray

I am much obliged for your letter which has been very interesting to me. — Your "indefinite" answers are perhaps not the least valuable part; for Botany has been followed in so much more philosophical spirit than Zoology, the I received like to hear any general remarks on Zoology without I find the Botanists will report to intermediate varieties cases. Thus it puts, as I suspected, much being true, I found the similitudes & diffs which too strongly (without the you point out) of a very good naturalist, viz. Wallaston, in regard to insects; & if the cut be established as true it will, I think, be a curious point. — Your answer in regard to introduced plants not being particularly variable, agrees with ✱ an answer, which Mr. H. C. Watson

has sent me in regard to British Aquigion plants, & said (whether or no naturalized) are not found only in cultivated land. It seems to me very odd, without any theoretical notions of any kind, that such plants should not be variable; but the evidence seems against it. —

Very sincere thanks for your kind invitation to the U. States: in truth there is nothing which I shall enjoy more; but my health is not, & will, I suppose, never be strong enough, except for the quietest routine life in the country.

I shall be particularly glad of the sheet of your paper on Geographical Distribution; but it really is unlikely in the highest degree that I could make any suggestions. —

With respect to my remark that I supposed that there were but few plants common to Europe & U. States, not ranging to the Arctic Regions; it was founded on vague

grounds, & partly on range of animals. But I took H.C. Watson's Remarks (1835) & in the latter end I found that out of 449 plants believed to be common to the Old & New worlds, ~~I found~~ of 110, ~~that~~ did not range on either side of the Atlantic up to Arctic region. And in writing to Mr. Watson, to ask whether he knew of any plant not ranging northward of Britain (say 55°) which was in common, he writes to me that he imagines there are very few; with Mr Symes' assistance he found some 20–25 species thus circumstanced, but many of them, from one cause or other, he considered doubtful. As examples, he specifies to me, with doubt, Chrysosplenium oppositifolium; Isoetes palustris; Astragalus hypoglottis; Thlaspi alpestre; Arenaria verna; Lythrum hyssopifolium. —

I hope that you will be inclined to work out for your next Paper, what number of your 321 in common do not range to Arctic Regions. Such plants seem exposed to such much greater difficu

in diffusion. — My very thanks for all your kindness & answers to my questions, & believe me

Your very sincerely & obliged

Ch. Darwin

If anything should occur to you on variability of Rhododendron & aragarian plant; I hope that you will be so kind as to let me hear, as it is a point which interests me greatly.

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Dr Asa Gray
Cambridge
Massachusetts
U.S.

Down Bromley Kent

Oct. 12th

My dear Dr. Gray

I received yesterday your most kind letter of the 23d. & your "Statistics" a two days previously another copy. I thank you cordially for them. Botanists write, of course, for Botanists; but as far as the opinion of an "outsider" goes, I think your paper admirable. I have read carefully a good many papers & works on Geograph. Distribution, & I know of any one Essay (viz Hooker's N. Zealand) that makes any approach to

subject very instructive.

Nothing has surprised me more than the greater generic & specific affinity with E. Asia than with W. America. Can you tell me (& I will promise to inflict no other questions) whether climate explains this greater affinity? or it is one of the many utterly inexplicable problems in Bot. Geography? Is E. Asia nearer as well known as West America? so that does the state of knowledge allow a pretty fair comparison? I presume it would be inexplicable, but I think it would make in one point your latter of generic ranges more clear (admirably clear as they seem to me)

the charm with which your paper makes a ~~man~~ non-Botanist appreciate the character of the flora of a country. It is wonderfully condensed (what labour it must have required!): you ask whether such details are worth giving, in my opinion there is literally not ~~one~~ word too much. ~~& so~~

I thank you sincerely for the information about "social" & "staying plants"; & likewise for giving me some idea about the proportion (ie 1/4), of European plants, which you think do not range to the extreme north: this proportion is very much greater than I had anticipated from what I picked up in conversation &c. —

To return to your statistics: I daresay you will give how many genera (& orders) your 260 introduced plants belong to: I see they include 113 genera non indigenous: as you have probably a list of the introduced plants, would it be asking too great a favour to send me per Hooker or otherwise just the total number of genera & orders to which the introduced plants belong: I am much interested on this, & have found De Candolle's remarks on the

their separate copies of papers with the proper pages for reference. Is not this good scheme & worth Prof. Silliman's attending to? In a reference in body of your own paper I have corrected your paging.

Hooker has lately returned from his continental trip & I am going to see him on Friday. Have you seen his Review on Decandolle: I cannot but think he is rather too severe on want of originality, & I hope much too severe on whole great & noble subject of Bot. Geography. — With most sincere & hearty thanks for all your great kindness. Your very truly

C. Darwin



If you could show, even roughly, what proportion of the genera in common to Europe (i.e. nearly half) are very general or mundane types; as your results now stand at first glance the affinity seems so very strong to Europe, owing, as I presume to the (nearly) half of the genera including very many genera common to the world or large portions of it. Europe is then unfairly excluded. — Is this not so? If we had the number of genera strictly or nearly strictly European, one could

compare better with Asia &
Southern America &c. But
I daresay this is a Utopian
wish owing to difficulty of tracing
what genera to call mundane.
Not here I my ideas at
all clear on subject, & I
have expressed them either less
clearly than I have them.

I am so very glad that you
intend to work out the range
of the 321 European species; for it
seems to me to be far most
important element in this
distribution. —

And I am equally glad that
you intend to work out range
of species in regard to size of
genera i.e. number of species in
genus. — I have been attempting
to do this in a very few cases;
but it is folly for any one but
Bentham to attempt it: I think
that De Candolle has
fallen into error in attempting to
do this for orders instead of for
genera, — for reasons which I will
not trouble you. —

(vol. 229)
In second column Heading p. 27 then
is misprint "and," for "not", which
might seriously mislead an idle
reader who only looked at
general totals.

Many of our Societies always page

which other & poorer workmen cannot
even perceive, so you cannot
appreciate your own work in
the generalising view. — Good
Heavens if I had written
a paper half as good
as yours, how conceited
I should have been!
With my hearty thanks
for all your kindness, & —
I have just been looking
over some of your old
letters — Believe me
Yours most truly
C. Darwin

I cannot get over my surprise at your maintaining a aquatic plant not being variable (I do not mean of this preserving marked races) — Pray keep this point in mind.

5

Down Bromley Kent
Nov. 24th

My dear Dr. Gray

Although I have nothing particular to say, yet I cannot resist thanking you warmly for your letter of Nov. 4th. — Your facts on northern range have indeed astonished me; they will be pre-eminently useful for my especial purpose.

I forwarded your enclosure to Lyell day of this receipt. —

I am delighted to hear that you intend to attack the naturalised plants, with especial

case — I have asked (indeed I asked it many months ago) your new Edit. & hope soon to get it. —

The last sentence in your letter at first surprised me & troubled me to a degree which would have made you laugh, had you seen me. viz "the a considerable part of our Alpine plants are not known in our Arctic continental regions". I did not perceive that you had added ~~any~~ but on connected with Scandinavia though Labrador so. — And this made me happy again. But looking at the Flora in it not rather a forced expression to exclude Labrador from your "Arctic continental regions"? — I try think of this, as you may confound some one else as you did me. When you say the only method to make your work in "to show you the way" — it convinces me of one thing alone, — that, as to my best workman sees blemishes in his work

hearing some careful observation opposed to this, said he did not believe it, "for Nature never lied" — I am just in this predicament & repeat to you that "Nature never lies!" ergo, therefore are always right. —

In reading your paper, one point struck me as well worth working out, if it could be done, viz a comparison of the principal Zone of habitation



Down Bromley Kent
Jan. 1st

My dear Dr. Gray

I have received the 1st part of your paper, & though I have nothing particular to say, I must send you my thanks & hearty admiration.

The whole paper strikes me as quite exhausting the subject, & I quite fancy & flatter myself I now appreciate the character of your Flora.

What a difference in regard to Europe from climates in

relation to the genera makes!

I have been exceedingly glad to see your conclusions in regard to species of large genera widely ranging: it is in strict conformity with the results I have worked out in several ways. It is of great importance to my notions. By the way you have paid me a <u>great</u> compliment: to be <u>mightily</u> mentioned even in such a paper, I consider a very great honour. — One of your conclusions makes me groan, viz that the line of connection of the strictly alpine plants in through Greenland: I shd. <u>extremely</u> like to see your reasons published in detail, for it "jiles" me (this is a proper expression; is it not?) dreadfully. — Hooker told me, that he spoke having a theory about when Saxifrages were first created, or

is very striking, & the more so, as he makes 3 classes of Trees, Bushes, & herbaceous plants. — (He says further he thinks the Tasmanian Flora on same principle.) The Bushes hold intermediate position between the other two classes. — It seems to me a curious relation in itself, & is very much so, if my theory & explanation are correct. With kind thanks for most interesting paper

Yours truly
Ch. Darwin

Nov. 30 not forget variability of Naturalized plants! —

in the U. States of the 320 European plants in comparison with your 115 representative species & the 15 strongly marked varieties = 130 species; & there again widely with the (4th & 5th) classes of strictly congeneric & perhaps divergent congeneric species. — I should be extremely if you do not get a very curious & harmonious result, i.e. on the great principle

the Nature here lies. — Overwhelmed as you are, I dare say you will say that I am an odious plagueer; — but here is another suggestion! I was led by one of my wild speculations to conclude (though it has nothing to do with geography. ~~[illegible]~~ distribution, yet it has with Flora) that Trees would have strong tendency to have flowers either diœcious, monœcious or polygamous structure. Seeing that this seemed so in Persons, I took our little British Flora, & discriminating trees from Bushes according to London, I have found that the result was in spring, genera a Families, as I anticipated. So I sent my notes to Hooker to ask him to tabulate his Jodrell Flora for this end, & he thought my result sufficiently curious (to do so); & the accordance with Britain

[Ras]

P.S.

You might give me a valuable piece of information, with very little trouble to yourself. — I have been comparing, as far as I can, Portean genera, & have left off in a maze of perplexity. By Portean genera, I mean such as hardly two Botanists agree in about to species, — what to call species & what varieties. Now what I want to know is, whether such genera as Salix, Rubus, Rosa, Mentha, Saxifraga, Hieracium, Myosotis, have equally Portean species in U. States, or if they have only one, but more especially if they have many. I think you have no Rosa, &

I forget how it is with some of the other genera. — The common case wd. be equally valuable to me if you would think over your half-dozen or dozen worst genera which have any European species, or then I could find out whether such are very troublesome in Europe. — I think Hooker told me that in Himalaya, Rubus & Salix, though large genera, were not troublesome to make out. — I think Potamogeton of sedges are troublesome at all seasons & times & in all places.

Down Bromley Kent

My dear Dr. Gray (I have divided my letter to save postage)

Your last letter, like all its predecessors has been very valuable to me; & every word on it has interested me. When I said that your remarks on your alpine plants "riled" me; I did not mean to doubt them, except in the Aggassian sense that they went against some theoretic notions of mine. Your notions are too long to give & indeed not worth giving, as far as America is concerned, & I can see from your letter that we shall take very much the same view. I am very glad to hear that you think of discussing the relation ranges

of the identical & allied U. States & European species, when you have time. Now this leads me to make a very audacious remark in opposition to what I imagine Hooker has been writing to you & some other scientific conscience. I presume he has been urging you to finish your great Flora, before you do anything else. Now I would say it is your duty to generalize as far as you safely can from your as yet completed work. Undoubtedly careful discrimination of species is the foundation of all good work; but I must

look at such papers as yours in Silliman as to fruit. As careful observation is far harder work than generalisation & still harder than speculation; do you not think it very possible that it may be overlooked? It ought never to be forgotten that the observer can generalize his own observations in comparably better than anyone else. How many astronomers have laboured thro' whole lives at observations & have not drawn a single conclusion; I think it is Herschel who has remarked how much better it would be, if they had paused in their dull work & seen what they could have deduced

from their work. So do pray look at this side of question, & let us have another paper or two like the last admirable ones. There, am I not an audacious dog!

You ask about my doctrine which led me to expect that Trees would tend to have separate sexes. I am inclined to believe that no organic being exists which perpetually self-fertilises itself. This will appear very wild, but I can venture to say that if you were to read all my observations on this subject, you would agree it is not so wild

as it will at first appear to you, from flowers said to be always fertilized in bud &c &c. — It is a long subject to which I have attended to for 18 years! Now it occurred to me that in a large tree with hermaphrodite flowers, we will say it will be ten to one that it would be fertilized by the pollen of its own flower, & a thousand or ten-thousand to one that if crossed, it would be crossed only with pollen from another flower of same tree, which would be opposed to my doctrine. Therefore on the great principle of "nature abhorring" I fully expected that trees

would be apt to be diœcious or monœcious (which as pollen has to be carried from flower to flower sometimes, would favour a cross from another individual of the same species) & so it seems to be in Britain & N. Zealand. But can the fact be explained of certain families having this structure & choosing to be trees, for the ~~[illegible]~~ rule seems to hold both in genera & families, as well as in species.

I give you full permission to laugh your fill at this wild speculation; & I do not

pretend to what it may be chance
which, in this case, ~~about~~ has
led me apparently right. But
I repeat that I feel sure that
my ~~case~~ doctrine has more
fertility, than at first it
appears to have. If you had not
asked, I sh. not have written
at such length, though I cannot
give any of my reasons.
The Leguminosæ are my greatest
opposers; yet if I were to that
to observations on insects made
during many years, I sh. fully
expect cross to take place in them;
but I cannot find the same
garden varieties ever cross
each other. I do not ask

you to take any trouble about it,
but if you should by chance come
across any intelligent nurserymen,
I wish you w^d. enquire whether
they take any pains in raising
to vars. of papilionaceous plants
apart to prevent crossing. The
worst is that nurserymen
are apt to attribute all
variation to crossing. —
Finally I incline to believe
that every living being varies
with distinct individuals
an occasional cross & as
titles from mere multitudes
of flowers offer obstacle to this,
I suspect this obstacle is counteracted

[I have seen statement of naturally formed cross of Phaseolus vulgaris]

18.

of tendency to have sexes separated.
But I have forgotten to say that
my maximum difficulty is trees
having papilionaceous flowers: some
of them, I know, have their ~~flowers~~
keel-petals expanded when ready for fertilization;
but Bentham
does not believe that this is general:
nevertheless on principle of nature
not lying I suspect that this
will turn out so, or that
they are eminently sought for
Bees dusted with pollen. Again
I do <u>not</u> ask you to take trouble,
but if strolling under your
Robinias when in full flower, just
look

at stamens & pistils better
pollinated & better Bees visit
them. — I must just mention
a fact mentioned to me the
other day by Sir W. Macarthur,
a clever Australian gardener,
viz how odd it was that
his Erythrinas in N.S. Wales
would not set a seed, without
he imitated the movements of
the petals which Bees
cause. — Well, as long
as you live, you will never
after this fearfully long
note ask me why I
believe this or that.

I am <u>particularly</u> obliged for your information about Potean genera; as this is one of the greatest of my many great puzzles; viz to know or conjecture whether the great variability of such genera is due to their conditions of existence, or whether it is apt to be inherent in them at all times & places (I am aware that this cannot be strictly predicated of any genus, for we have some fixed species). — Now I have thought that you would not object to my sending the latter half of your note with list of such American genera to Mr. H. C. Watson, of whose great clearness of mind & acuteness I have from long correspondence the <u>highest</u> opinion. — I have sent his

note (1) & Eq of (2) for the chance of your liking to see them; but if more busy even than usual, do not read them; & I dare say in course of half-year, you will be able to return them together with your own list; but pray do not lose them. There is one point, to which for myself, I wish to call your attention, viz. lest you rightly understood that my question did not refer to genera having very close species, but to genera having very variable species. Watson thought that you might not have understood me. — If you do read Watson's papers & have anything to remark on subject, I need not say, how very much I shall like to hear it. I am sure I

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do not know whether I have acted (4
wisely in sending Watson letters,
but I repeat again pray do not
read them, without you feel
inclined: as, I suppose, in
course of few months you would
be sending some parcel to
England, it will not cost you
much to return them.
With hearty thanks for all your
kindness, & begging forgiveness for
length of this letter, which is
chiefly your own fault (as far
as I am concerned), believe
me, yours most sincerely
C. Darwin

By my <u>very</u> expectation the species having diminished ranges would belong to <u>small</u> genera; so you may imagine how much interest I felt in coming on your note on this my subject. —

Your list of the Trees made by months rather worse to know what proportion had seeds in some degree separated; on which subject

Down Bromley Kent
May 4th

My dear Dr. Gray

I must thank you for your new part of Statistics: if feeling the most lively interest in reading it can make me worthy of receiving it, assuredly I am worthy. I will not trouble you by specifying the many points which have particularly struck me; but the note at p. 387 I must allude to, as I want to

ask a question which I think
it cannot take 5 minutes to
answer, namely to how many
genera the 49 species belong:
as there are in Carey there
cannot be more than 44: I
tried to go through list, but
I ∁ not feel sure in
separating the 3d head from 1st
& 2d Heads. – I want to
know to see more clearly
in proportion to genera, where flora
how large the proportion of
monotypic genera are in

the disjoined species. This
subject interests me very much;
I began to try to work
out this point in all the
cases of much disjoined
species which I met with;
but I failed from want of
knowledge: I tried also to
make out whether the
disjoined species would not on average
belong to small families,
but here again I failed from want of knowledge, though
the cases in which I could
find out something, confirmed

to be, I do think, the fox
trot in same latitude,
& not the standard of
the individual country.
Though why I should trouble
you with an old
exploded notion of
mine, I know not.
With my sincere thanks
& good wishes —
Believe me
Yours most sincerely
Ch. Darwin

1879

I wrote you a ridiculously
long letter some weeks
ago.
I am so glad that you
are going to attack
Jane introduced plants
in the next number:
I may mention that two
or three years ago I
compared the proportions of
the British introduced
species to the native

Flora & it was in several cases ridiculously close: I then took some first Edition & did the same, but the proportions here were very different; but I think the point will be just worth looking to, for chance of some ~~notice, for~~ result.

I have just looked at my list usefully later, & I see I made out in some manner 206 introduced plants & of them Compositae (as I thought) fourteen 1/8 & 10 do, some indigenous Compositae. —

In Britain for the whole Cybele

| | Introduced | Indigenous |
|---|---|---|
| Compositae | 1/10 | 1/9 |
| Umbelliferae | 1/22 | 1/23 |
| Labiatae | 1/28 | 1/30 |
| Leguminosae | 1/20 | 1/18 |

I happened to show the other results first, & was inclined to think something of them; but I suppose all, chance & the standard proportions ought

9A

Down Bromley Kent

June 18th —

My dear Dr Gray

I must thank you for your two very valuable letters. It is extremely kind of you to say that my letters have not bored you very much, & it is almost incredible to me, for I am quite conscious that my speculations run quite beyond the bounds of true science. One chief object of this note is to say that I have not received the last part of your Silliman Papers: Hooker has, & says he will lend it me, if, as is my

lately, you have not another copy.
But it may come with the
Wallace correspondence. — Your
remarks on the head side
have of <u>real</u> use to me
when I return to the subject,
for a man must be blind
not to see how cautious
a reasoner you are.

Thank you much for your remarks
on disjoined species: I daresay
I may be quite in error:
I saw so much difficulty even
<u>theoretically</u> & so much impossibility
practically from my ignorance, that
I had given up notion till I

read your note to your article.
I had just copied out a few
striking cases out of Hooker's
Himalayan Journal & turned to
Wallace to see what the
general rule was. The notion
was grounded on the belief
that disjoined species had
suffered much local extinction
& therefore [conversely with the
case of genera with many
species being species with wide
& continuous ranges.] I infer that genera
with very few species (i.e. from extinction)
would be apt (not necessarily always) to have
narrow ranges & disjoined ranges.
You will not remember, perhaps,

that I am driving at & it is
not worth enlarging on, — but I
look to extinction as a main
cause of <u>wide</u> genera & <u>their joined</u> ranges & therefore
They ought if they behaved
properly & as nature does let them to go together! —

I have not the least doubt that the proportion of naturalised British plants was due to simple chance; but I thought it was just worth mentioning to you: I had from your former Edition of Manual quite given up idea. —

It has been extremely kind of you telling me about the trees: now with your facts, & those from Britain, N. Zealand, & Tasmania I shall have fair materials for judging: I am writing this away from home, but I think your fraction of $\frac{95}{132}$ is as large as in other cases, & is at least a striking coincidence. —

I thank you much for your remarks about my crossing notions, to which I may add, I was led by exactly the same idea as yours, viz the crossing

must be one means of eliminating variation, & here I wished to make out how far in animals & vegetables this was possible. — Papilionaceous flowers are almost dead flowers to me, & I consider experimentation as castration alone, often produces sterility.

I am surprised at what you say about Compositae & Gramineae. From what I have seen of latter they seemed to me (& I have watched wheat owing to what L. de Longchamps has said on their fertilisation in bud) ~~eminently~~ favourable for crossing — & from Cassini's & Kölreuter's on the Compositae & C.C. Sprengel's I had concluded that these were eminently likely

[I am aware of the point looking at pollen.]

to be expected. If in some months time you can find time to tell me whether you have made any observations on the early fertilization of plants in these two orders, I, for one, will be very glad to hear, as it will save me from great blunders. In several published remarks in various general on this subject, it has seemed to me that the early fertilization has been inferred from the early shedding of the pollen, which I think is clearly false inference. Another cause, I, for think, of the belief of fertilization in the bud, is the not-rare abnormal ~~[illegible]~~ early maturity of the pistil, as

described of Goethe. — I have hitherto failed in meeting with detailed accounts of a regular & normal impregnation in the bud. — Indeterminate & (& Leguminosæ) undeg. water seem & are strongest cases against me, as far as I am yet know.

I am so sorry that you are so overwhelmed with work; it makes you a very great kindness to me the more striking. Believe me. Yours gratefully

C. Darwin

It is really pitty to see how effectual insects are: a short time ago I found female Holly 60 measured yards from any other Holly & I cut off some twigs & took of hence 20 stigmas, cut off their tops & put them under microscope: there was pollen on every one & in profusion on most. Weather cloudy & stormy & unfavourable, wind in wrong direction to bring bees.

Down Bromley Kent July 20th

9 B

My dear Dr Gray

What you say about extinction, in regard to small general & local ~~divide~~ distinction, being hypothetical seems very just. Something direct, however, could be advanced on this head from fossil shells; but hypothetical such notions must remain. It is not a little egotistical, but I should like to tell you, (& I do not think I have) how I view my work. Nineteen years (!) ago it occurred to me that whilst otherwise employed on Nat. Hist, I might perhaps do good if I noted any sort of facts bearing on the question of the origin of species; & then I have since been doing. Either species have been independently created, or they have descended

from other species, like varieties from one species. I think it can be shown to be probable that man gets his most distinct varieties best worth keeping by preserving such as arise, & destroying the others, — but I th^k^ I'll write a article if I can to go on. To be brief I assume that species arise like our domestic varieties with much extinction; & then test this hypothesis of comparison with as many general & pretty well established propositions as I can find out, — in geograph. distribution, geological history — affinities &c &c. And it seems to me, that supposing that

such hypothesis was to explain such general propositions, we ought, in accordance with common way of filling all sciences, to admit it, till some better hypothesis be found out. For to my mind to say the species were created so & so is no scientific explanation of a transient way of saying it is so & so. But it is unscientific trying to show how I try to proceed in consequence of a note. But as an honest man I must tell you that I have come to the deliberate conclusion that there are no such things as independently created species — that species are only strongly defined varieties. I know that this will make

you despise me. — I do not mind
underrate to many <u>huge</u> difficulties
on this view, but yet it seems
to me to explain too much,
Mercurius inexplicable, to be
false. Just to allude to one
point in your last note, viz
about species of the same genus
<u>generally</u> being a common or
continuous area; if they are
actual lineal descendants of
one species, this of course would
be the case; & the sadly too
many exceptions (for me) how to
be explained of climates &
geological changes. A fortiori as
(at a early same ground) the individuals of
this view & all the
the same species. If have a
continuous distribution. On this

latter branch of the subject I have put a chapter together, & Hooker kindly read it over: I thought the exceptions & difficulties were so great that on the whole the balance weighed against my notions, but I was much pleased to find that it seemed to have considerable weight with Hooker, who said he had been so much staggered about the permanence of species. —

I must say one word more in justification (for I feel sure that your tendency will be to despise me & my conclusions) that all my notion about how species changes are derived from long-continued study & (& conversing with) agriculturists & horticulturists; & I believe I see

I say pretty clearly on the means used by Nature to change her species & adapt them to the conditions & exquisitely beautiful contingencies to which every living being is exposed.

Thank you much for what you say about the spitting & crossing of the grapes: I have been often astonished at what Bokenham says on fertilization in the bud: I have seen Cruciferae mentioned as in time, which every gardener knows for difficult it is to protect from crossing!

What you say on Papilionaceous flowers is very true; & I have no doubt that varieties are crossed; but yet (& the same remark is applicable in a beautiful way to Fumaria & Dicentra) as I noticed

many years ago) I must believe that the flowers are constructed partly in direct relation to the visits of insects; & no insect can avoid bringing pollen from other individuals. I cannot understand. It is really pretty to watch the action of a Humble-Bee on the scarlet Kidney Bean, & in this genus (& in Lathyrus & green-house) the ~~[illegible]~~ honey is so placed that the Bee inevitably alights on the side of the flower towards which the spiral pistil is protruded (bringing out with it pollen) & by the depression of the wing-petal is forced against the Bee's side all dusted with pollen. In the Broom the pistil is rubbed on centre of back of Bee &c. &c. — I suspect then

N.B. If you will look at bed of Scarlet-Kidney Bean you will find the wing-petal on the left-side above on all detached & the basis of the Bees.

is something to be made out about the Leguminosæ which will bring the case within our theory; though I have failed to do so. Your theory will explain why in vegetable & animal kingdoms the act of fertilisation even in hermaphrodites usually takes place sub-julice, though thus exposed to the greatest injury from damp & rain. In animals in which the sexes occasionally cohabit & insects or wind; then in no case of land-animals being hermaphrodite without the concurrence of two individuals. But my letter has been horribly egotistical: but your letters always so greatly interest me; & what is more they have in simple truth been of the utmost value to me. Good bye & believe me gratefully

C. Darwin

90-2125

Letter from Charles Darwin to Asa Gray, 1856, discussing his developing ideas concerning the origin and mutation of species.

Down Bromley Kent July 20 (1856) 1857

My dear Dr. Gray:

What you say about extinction, in regard to such genre and local disjunction, being hypothetical seems very just. Something direct however, can be advanced on this head from fossil shells; but hypothetical such notions must remain. It is not a little egotistical, but I should like to tell you, (and I do not think I have) how I view my work. Nineteen years (!) ago it occurred to me that whilst attention employed on Natural History I might perhaps do good if I noted any sort of facts bearing on the question on the origin of species; and this I have since been doing. Either species have been independently created, or they have descended from other species, like varieties from one species. I think it can be shown to be probable that man gets his most distinct varieties by preserving such as arise best worth keeping and destroying the others, but I should fill a quire if I were to go on. To be brief I assume that species arise like our domestic varieties with much extinction; and then test this hypothesis by comparison with as many general and pretty well established propositions as I can find made out, in geographic distribution, geological history, affinities etc. etc. etc. And it seems to me, that supposing that such hypothesis were to explain such general propositions, we ought in accordance with common way of following all sciences, to admit it, till some better hypothesis be found out. For to my mind to say the species were created so and so is no scientific explanation but a prescient(ific) way of saying it is so and so. But it is not sensible trying to show how I try to proceed in compass of a note. But as an honest man I must tell you that I have come to the relentless conclusion that there are no such things as independently created species, the species are only strongly defined varieties. I know that this will make you despise me. I do not much underrate the many huge difficulties on this view, but yet it seems to me to explain too much, otherwise inexplicable, to be false. Just to allude to one point in your last note, viz about species of the same genus generally having a common or continuous area: if they are actual lineal descendents of one species, this of course would be the case; and the sadly too many exceptions (for me) have to be explained by climactic and geological changes. A fortiori on this view (but on exactly same grounds) all the individuals of the same species should have a continuous distribution. On this latter kind of subject I have put a chapter together and Hooker kindly read it over: I thought the exception(s) and difficulties were so great that on the whole the balance weighed against my notions, but I was much pleased to find that it seemed to have considerable weight with Hooker, who said he had never been so

much staggered about the permanence of species. I must say one word more in justification (for I feel sure that your tendency will be to dispare over my contents) that all my notions about how species change are derived from long continued study of the works of (and concern with) agriculturalists and horticulturalists; and I believe I see my way pretty clearly on the means used by nature to change the species and adapt them to the conditions and exquisitely beautiful contingencies to which every living being is exposed.

I thank you much for what you say about variability and crossing of the grasses: I have been often astounded at what Botanists say on fertilization in the bud: I have seen Cincifera mentioned as instances, which every gardener knows how difficult it is to protect from cuping! What you say on Popilionaceous flowers is very true; and I have no facts to show the varieties are cuped; but yet (and the same remark is applicable in a beautiful way to Frumaria and Dielytia as I noticed many years ago) I must believe that the flowers are constructed partly in direct relation to insects' visits; and how insects can avoid bringing pollen from other individuals I cannot understand. It is really pretty to watch the action of a Humble-Bee on the scarlet Kidney Bean, and in this genus (and in Lathrus Grand. flowers) the honey is so placed that the Bee invariably alights on the side of the flower towards which the pistol is pointed (bringing out with it pollen) and by the depression of the wing-petal is forced against the Bees' side all dusted with pollen. In the Broom the pistol is rubbed on centre of back of Bee. I suspect there is something to be made out about the Leguminosae which will bring the case within our theory: though I have failed to do so. For theory will explain why in vegetable and animal Kingdoms the act of fertilization even in hermaphrodites usually takes place sub-jove, though thus exposed to the great injury from damp and rain. In animals in which the semen cannot, like pollen be occasionally carried by insects or wind: there is no case of Land-animals being hermaphrodite without the concourse of two individuals. But my letter has been horribly egotistical: but your letters always so greatly interest me; and what is more they have in simple truth, been of the utmost value to me.

Yours most sincerely and gratefully

C. Darwin

Gray Papers, Harvard University. Published in Nathan Reingold, ed., Science in Nineteenth-Century America (New York, 1964), 187-189.

Down Bromley Kent
Sept. 5

My dear Gray

I forget the exact words which I used in my former letter, but I daresay I said that I thought you would utterly despise me, when I told you what views I had arrived at, which I did because I thought I was bound as an honest man to do so. I fear here been a strange mortal, seeing how much I owe to your quite extraordinary kindness, if in saying this I had meant to attribute the least bad feeling to you. Permit me to tell you, that before I had ever corresponded with you, Hooker had shown me several of your letters (not of a private nature) & these gave me the warmest feeling of respect to you; & I fear indeed be ungrateful if your letters to me & all I have heard of you, I have been lately at work on a point which interests me much; namely dividing the species of several Floras into two as nearly as equal cohorts as possible — one with all those in forming large genera, & the other with the small genera. Thus in your N. States Flora, I make (with misgivings for a few problem genera & lately for its unusual size) 1005 sp. in genera of 5 & upwards, & 917 in genera with 4 & downwards; & the large genera have 88/1000 varieties & the small genera only 50/1000. — This rule seems to be general. & Hooker is going to work out some Flora on same plan. — But to my disgust was nearly marked of big-typhus 48/1000 to 46/1000 are only in proportion.

I have, also, lately been observing daily Lobelia fulgens — this in my garden is never visited by insects & never sets seed, without pollen be put on stigma. (whereas to make blue Lobelia is visited by bees & does set seed); I mention this because there are such beautiful contrivances to prevent the stigma ever getting its own pollen; which seems only explicable on the doctrine of the advantage of crosses.

I forget whether I ever said I had received safely Mr. Watson's papers. a few days before in Bromley, for which my many thanks & Bot I have never had the least part of your paper on Naturalised Plants. If you have a spare copy (which is not likely) I shall be very glad of it: otherwise I will borrow Hooker's. I ought to feel ashamed of the length of this letter, knowing how busy you are.

My dear Mr. Gray
Believe me with much sincerity. Your truly
C. Darwin

I will try if I can anywhere get seed of the Adlumia cirrhosa & shall be sure to do it next summer. Perhaps they have it at Kew.

had not strongly enhanced this feeling. But I did not feel in the least sure that when you knew whither I was tending, that you might not think me so rash & foolish in my views (God knows I arrived at them slowly enough, & I hope conscientiously) that you would think me worth no more notice or criticism. To give one example, the last time I saw my dear old friend Falconer, he attacked me most vigorously, but quite kindly, & told me "you will do more harm than any ten naturalists will do good" — "I can see that you have already corrupted & half-spoiled Hooker" (!!). Now when I see such strong feeling in my ablest friends, you need not wonder that I always expect my views to be received with contempt. But enough & too much of this. —

I think you must truly forgive the kind spirit of your last letter. I agree to every word in it; & think I go as far as almost anyone in seeing the great difficulties against my doctrine. With respect to the extent to which I go, all argument in favour of my notions fade rapidly away the greater the scope of forms considered. But in animals, embryology leads me to an enormous & frightful length. The facts which kept me longest scientifically orthodox are those of adaptation — the pollen-mass in Asclepias — the mistletoe, with its pollen carried by insects & birds — the woodpecker with its feet & tail & tongue to climb tree & secure insect. To talk of climate or habit producing such adaptations to these organic beings is futile. This difficulty, I believe I have remarked. As you seem

Several things, seen, made me confidently believe that "close" species occurred most frequently in the larger genera; & you may remember that you made me the enclosed list. Now to my utter disgust, I find that the case is somewhat the reverse of what I had so confidently expected, the close species hugging the smaller genera. Hence I have enclosed a list. & beg you kindly to run your eye over it, & see whether, not understanding my notion, you did have attended more to the small than to the large genera: but I can see that this is not probable. And do not think that I want you to "cook" the results for me. — Are the close species usually geographical representative species: this might make some difference?

Lately I examined buds of Kidney Bean with pollen shed, but I could not believe that the pollen is kept on the stigma by cold or otherwise, except if Bees visiting & moving the pistils: hence I included a small bunch of flowers in two Bottles, in every way treated the same: the flowers in one I daily just momentarily moved as if by a Bee; then set 3 fine pods, the other not one. Of course this little experiment must be tried again, & this year in England it is too late, as the flowers seem very seldom to set. If Bees are necessary to the flower's self-fertilisation, Bees must almost kiss them, as they touch the stigma.

Down Bromley Kent

Sept. 5th

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I shd have been a strange mortal, seeing how much I owe to your quite extraordinary kindness, if in saying this I had meant to attribute the least bad feeling to you. Permit me to tell you, that before I had ever corresponded with you, Hooker had shown me several of your letters (not of a private nature) & these gave me the warmest feeling of respect to you; & I shd indeed be ungrateful if your letters to me & all I have heard of you, had not strongly enhanced this feeling. But I did not feel in the least sure that when you knew whither I was tending, that you might not think me so wild & foolish in my views (God knows arrived at slowly enough, & I hope conscientiously) that you would think me worth no more notice or assistance. To give one example, the last time I saw my dear old friend Falconer, he attacked me most vigorously, but quite kindly, & told me "you will do more harm than any ten naturalists will do good"— "I can see that you have already corrupted & half-spoiled Hooker"(!!). Now when I see such strong feeling in my oldest friends, you need not wonder that I always expect my views to be received with contempt. But enough & too much of this.—

I thank you most truly for the kind spirit of your last letter. I agree to every word in it; & think I go as far as almost anyone in seeing the grave difficulties against my doctrine. With respect to the extent to which I go, all arguments in favour of my notions fall rapidly away the greater the scope of forms considered. But in animals, embryology leads me to an enormous & frightful range. The facts which kept me longest scientifically orthodox are those of adaptation—the pollen-masses in Asclepias —the misseltoe with its pollen carried by insects & seed by Birds the woodpecker with its feet & tail beak & tongue to climb trees & secure insects. To talk of climate or Lamarckian habit producing such adaptations to other organic beings is futile. This difficulty, I believe I have surmounted. As you seem interested in subject, & as it is an immense advantage to me to write to you & to hear ever so briefly, what you think, I will enclose (copied so as to save you trouble in reading) the briefest abstract of my notions on the means by which nature makes her species. Why I think that species have really changed depends on general facts in the affinities, embryology, rudimentary organs, geological history & geographical distribution of organic beings. In regard to my abstract you must take immensely on trust; each paragraph occupying one or two chapters in my Book. You will, perhaps, think it paltry in me, when I ask you not to mention my doctrine; the reason is, if anyone, like the Author of the Vestiges, were to hear of them, he might easily work them in, & then I shd have to quote from a work perhaps despised by naturalists & this would greatly injure any chance of my views being received by those alone whose opinion I value.—

I have been lately at work on a point which interests me much; namely dividing the species of several Floras into two as nearly as equal cohorts as possible—one with all those forming large genera, & the other with the small genera. Thus in your U. States Flora, I make (with omissions of naturalised & of a

few protean genera & Carex from its unusual size) 1005 sp. in genera of 5 & upwards, & 917 in genera with 4 & downwards; & the large genera have 881000 varieties & the small genera only 501000. This rule seems to be general. & Hooker is going to work out some Floras on same plan.— But to my disgust your vars. marked by big-type are only in proportion 481000 to 461000.

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Lately I examined buds of Kidney Bean with pollen shed, but I was led to believe that the pollen cd hardly get on stigma by wind or otherwise, except by Bees visiting & moving the wing petals: hence I included a small bunch of flowers in two Bottles, in everyway treated the same: the flowers in one I daily just momentarily moved as if by a Bee; these set 3 fine pods, the other not one. Of course this little experiment must be tried again, & this year in England it is too late, as the flowers seem now seldom to set. If Bees are necessary to this flower's self-fertilisation, Bees must almost cross them, as their dusted right-side of head & right legs constantly touch the stigma.

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I forget whether I ever said I had received safely Mr Watson's papers. & your Lesson in Botany, for which very many thanks & which I am now reading. But I have never had the last part of your paper on Naturalised Plants. If you have a spare copy (which is not likely) I shd be very glad of it: otherwise I will borrow Hooker's. I ought to feel ashamed of the length of this letter, knowing how busy you are.

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This sketch is most imperfect; but in so short a space I cannot make it better. Your imagination must fill up many wide blanks. — Without some reflexion it will appear all rubbish; perhaps it will appear so after reflexion. —

C. D.

This little abstract touches only on the accumulation of natural selection, which I look at as by far the most important element in the production of new forms. The laws governing the incipient or primordial variation (unimportant except as to groundwork for selection to act on, in which respect it is all important) I shall discuss under several heads, but I can come, as you may well believe, only to very partial & imperfect conclusions. —

I. It is wonderful what the principle of selection by Man, that is the picking out of individuals with any desired quality, and breeding from them, and again picking out, can do. Even Breeders have been astonished at their own results. They can act on differences inappreciable to an uneducated eye. Selection has been methodically followed in Europe for only the last half century. But it has occasionally, and even in some degree methodically, been followed in the most ancient times. There must have been, also, a kind of unconscious selection from the most ancient times, — namely in the preservation of the individual animals (without any thought of their offspring) most useful to each race of man in his particular circumstances. The "roguing" as nurserymen call the destroying of varieties, which depart from their type, is a kind of selection. I am convinced that intentional and occasional selection has been the main agent in making our domestic races. But, however, this may be, its great power of modification has been indisputably shown in late times. Selection acts only by the accumulation of very slight or greater variations, caused by external conditions, or by the mere fact that in generation the child is not absolutely similar to its parent. Man by this power of accumulating variations adapts living beings to his wants, — he may be said to make the wool of one sheep good for carpets and another for cloth &c. —

II. Now suppose there was a being, who did not judge by mere external appearance, but could study the whole internal organization — who never was capricious, — who should go on selecting for one end during millions of generations, who will say what he might not effect! In nature we have some slight variations, occasionally in all parts: and I think it can be shown that a change in the conditions of existence is the main cause of the child not exactly resembling its parents; and in nature geology shows us what

changes have taken place, and are taking place. We have almost unlimited time: no one but a practical geologist can fully appreciate this: think of the Glacial period, during the whole of which the same species of shells at least have existed; there must have been during this period millions on millions of generations.

III. I think it can be shown that there is such an unerring power at work, as Natural selection (the title of my Book), which selects exclusively for the good of each organic being. The elder De Candolle, W. Herbert, and Lyell have written strongly on the struggle for life; but even they have not written strongly enough. Reflect that every being (even the elephant) breeds at such a rate, that in a few years, ~~and~~ or at most a few centuries a thousands of years, the surface of the earth would not hold the progeny of any one species. I have found it hard constantly to bear in mind that the increase of every single species is checked during some part of its life, or during some shortly recurrent generation. Only a few of those annually born can live to propagate their kind. What a trifling difference must often determine which shall survive and which perish—

IV. Now take the case of a country undergoing some change; this will tend to cause some of its inhabitants to vary slightly; not but what I believe most beings vary at all times enough for selection to act on. Some of its inhabitants will be exterminated; and the remainder will be exposed to the mutual action of a different set of inhabitants, which I believe to be more important to the life of each being, than mere climate. Considering the infinitely various ways, beings have to obtain food by struggling with other beings, to escape danger at various times of life, to have their eggs or seeds disseminated &c. &c, I cannot doubt that during millions of generations individuals of a species will be born with some slight variation profitable to some part of its economy; such will have a better chance of surviving, propagating, this variation, which again will be slowly increased by the accumulative action of Natural selection; and the variety thus formed will either coexist with, or more commonly will exterminate its parent form. An organic being, like the woodpecker or mistletoe may thus come to be adapted to a score of contingencies: natural selection, accumulating those slight variations in all parts of its structure which are in any way useful to it, during any part of its life.

V. Multiform difficulties will occur to everyone on this theory. Most can I think be satisfactorily answered.— "Natura non facit saltum" answers some of the most obvious.— The slowness of the change, and only a very few undergoing change at any one time answers others. The extreme imperfections of our geological records answers others.—

VI One other principle, which may be called the principle of divergence plays, I believe, an important part in the origin of species. The same spot will support more life if occupied by very diverse forms; we see this in the many generic forms in a square yard of turf (I have counted 20 species belonging to 18 genera),— or in the plants and insects, on any little uniform islet, belonging almost to as many genera and families as to species.— We can understand this with the higher animals whose habits we best understand. We know that it has been experimentally shown that a plot of land will yield a greater weight, if cropped with several species of grasses than with 2 or 3 species. Now every single organic being, by propagating so rapidly, may be said to be striving its utmost to increase in numbers. So it will be with the offspring of any species after it has broken into varieties or subspecies or ~~born~~ true species. And it follows, I think, from the foregoing facts, that the varying offspring of each species will try (only few will succeed) to seize on as many and as diverse places in the economy of nature, as possible. Each new variety or species, when formed will generally take the places of and so exterminate its less well-fitted parent. This, I believe, to be the origin of the classification or arrangement of all

[Enclosure]

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turned out together on the Cumberland
mountains, & one particular breed
is found to succeed so much
better than the other,
that it fairly starves the other
to death: I should say
that natural selection picks
out this breed, & would tend
to improve it for abnormally
to have formed it. —

Many thanks for Sand & Heinrich
of Adhesion: I must confess
from what I have seen in Bees
whilst making a fortnight, I see no difficulty whatever
in Bees copying the individuals:
& I would venture to predict
that it has a nectary on both
sides instead of on only one side,



Down Bromley Kent
Nov. 29

My dear Gray

This shall be such an extraordinary
note as you have never received
from me, for it shall not contain
one single question or request.
I thank you for your inspection
of my views. Every criticism from
a good man is of value to me.
What you hint at generally is
very, very true, that my work
will be grievously hypothetical &
large parts of no more worth
of being called induction; my
commonest error being partly
induction from too few facts. —

I had not thought of your objection
of my using the term "natural
selection" as an agent; I
am it much as a personification
does the word Denudation, for
an agent, expressing the result
of several combined actions.
I wish I can to explain
not merely of inference, what I
mean of the term; for I
must use it, otherwise I shd
in apparently been to expand it
with some such formula as
the following, "the tendency to the
preservation (owing to the severe struggle
for life to which all organic
beings at some time or generation are
exposed) of any the slightest
variation in any part, which
is of the slightest use or favourable
to the life of the individual
which has then varied; together
with the tendency to its inheritance."
Any variation, which was of no
use whatever to the individual,
would not be preserved by the
principle of "natural selection". But
I wish not weary you by going
on; as I do not suppose I shd
make my meaning clearer without
large expansion. — I wish only add
one other sentence: ~~[illegible]~~
of several varieties of sheep have been

natural cause, to a species produced by the separate act of the Hand of God. But I am running on foolishly. — By the way I met the other day Phillips, the Palaeontologist, & he asked me "how do you define a species?" — I answered "I cannot" whereupon he said "at last I have found out the only true definition, — 'any form which has been called a species'" !

I am infinitely obliged to you for your offer (if you can ever find time, & how much overtasked you seem to be) of considering for the sake of case of finished plates can be packed with equal certainty both ways, but when there is only one necessary it can be packed (as far as I have seen) only the way: Lecoq, I observe, brings forward Francia as a genus which could never be confused by naturalists means, whereas I suspect it structure is formed in direct relation to farina crossing!!

I sent you Gardeners Chronicle with little notice on Kidney Beans: since writing it, I have received a most curious list of Beans naturally crossed, & the seed-coats affected by the ~~first~~

act of fertilisation like Gartner's
Pea case. — By the way I must
tell you what I heard yesterday,
though not in your line, but on
subject of the coupling of individuals.
Barnacles (Balanus) are hermaphrodite &
with their well shut up shell offer
as great a difficulty to coupling
as can well be conceived: I
found an individual with
monstrous & imperfect penis, but
yet with fertilised ova; but I
did not know whether it might
not be case of parthogenesis or
a strange accident of some
floating spermatozoa; yesterday I
had an account of a man who
watching some shells, saw one
fortunate enough to long protrude
penis, & insert it in the shell of an
adjoining individual! So here is a
load off my mind. —
You speak of species not having
any material base to rest on;
but is there any greater hardship
than deciding what deserves to be
called a variety & be designated
by a greek letter. When I was
at systematic work, I knew I
longed to have no other difficulty
than (great enough) deciding
whether the form was distinct
enough to deserve a name; & yet
to be haunted with undefined
& unanswerable question whether
it was a true species. Which
a fine is from a well
marked variety, produced by

My dear Gray

Yours heartily obliged

Darwin

How I wish I knew what large moth or Humble Bee (for large it must be) visits & fertilises Lobelia fulgens in its native home: do you know any southern Botanist who will look to this? I would cover a plant with a very coarse gauze cage, & then not a pod would set I believe. But you have broken my vow of a set of question or request!

again 9 Jan 18.

A list of close fliers, such as Hooker would perhaps keep together: you could not do me a more essential service.

If you do it, write on fleece lettr, if in your power, large & small orders as they come, for perhaps there may be some difference in the tube in large natural & small broken families.

I intend to go into this with Ledebour, as far as native varieties are concerned. In all Ledebour & many other Floras, I find the tube universal

of the large genera presenting most varieties. In the British Flora, of Mr Watson's aid, I have struck out the most trifling varieties & I find the rule holds good, as it also does with the forms which most British Botanists rank as species, but which I & other Botanists have considered as varieties. This rule, as I must consider it of the large genera varying most, I look at as most important for my work & I believe it to be the foundation of the manner in which all beings are grouped together in degree, with what I call the principle of divergence i.e. the tendency to the preservation & extinction of the most different members of each group. But I am annoying myself & scribbling away all my notions without any merit. Forgive me, & believe

"carefully the variation is large,
"or shall a very small
"genera?"

I know what fleeting & trifling things variations are; but my query applies to such as have been thought worth naming & recording. —

If you could screw time to send me even 10 brief words in answer to this, pray do so. It will be a great service to me. —

Yours most truly & obliged
Ch. Darwin



Down Bromley Kent
Feb. 21

My dear Gray

My last letter begged no favour, this one does; but it will really cost you very little trouble to answer to me. & it will be of very great service to me, owing to a remark made to me of Hooker, which I cannot credit, & which he suggested to him of one of my letters. He suggested by asking you, &

I told him. I would not
give the least hint
what he thought. I generally
believe Hooker implicitly,
but he is sometimes, I
think, & he confesses it,
rather over critical & his
ingenuity in discovering flaws
seems to me admirable. —
Here is my question —
"Do you think that God
" Admits in drawing up
" a local Flora, rather such
" a larger, or in making
a Prodromus like Decandolle,
would almost universally, but
unintentionally & unconsciously,
tend to record (i.e. marking
with greek letters & giving
short characters) varieties
in the larger or in the
smaller genera? Or would
the tendency be to record
the varieties about equally
in genera of all sizes?
Are you yourself conscious
on reflection, that you have
attended to, & recorded more

P. S. Gray 24

Do you know whether any one has ever published any remarks on the geographical range of varieties in plants in comparison with the species, to which they are supposed to belong. I have in vain tried to get some vague idea. I write to enquire of a little information on this head given me by Wahlenberg in a paper on Lapp Shells in U. States; I have quite failed; but perhaps it would

be difficult for you to give me even a brief one answer on the head, & if so I am not so unreasonable, I assure you, as to expect it. —

---

If you are writing to England now you could enclose the letter to me to forward.

---

Please address to questions in not letter then are more or fewer varieties in larger or smaller genera, but letter then is a stronger or weaker tendency in the minds of Naturalists to record such in large or small genera.

I shall not, you may depend, forget a request, which I look at as a favour. — But (& it is a heavy "but" to me) it will be long before I go to press: I can truly say I am never idle; indeed I work too hard for my much weakened health; yet I can do only 3 hours of work daily, & I cannot at the end, when I shall have finished: I have done eleven long chapters; but I have yet some other very difficult ones, on palæontology, classification & embryology &c. & I have to correct & add largely to the three done. I find, alas, each chapter takes me, or always takes me, three months, 10 times I am. There is no



Down Bromley Kent
Oct. 4th

My dear Gray

Your kindness to me is really beyond thanks. Believe me that I feel it. By an odd chance yesterday morning, before I got your letter, I had just written down what I had to say on closely allied species in large genera; & I thought that you had forgotten all about your list, & knowing how hard you were worked, to my credit be it said, I firmly resolved that nothing should induce me to remind you. Perhaps you may believe how delighted I was to get your list, which is now

being tabulated. — I am, also, partic=
=larly obliged for the answer to
my question: Hooker & several other
Botanists differ from you & think
there would be a strong tendency
to omit secondary actually exis=
=ting varieties in the smaller
genera. None of them pretend they
ever thought of this before I
asked the question, from which
little systematic work I have
done myself, I cannot reconcile
their views; & my letters, in
several respects contradict them
in my opinion, & show that
Botanists have worked rather
more systematically & regularly
in secondary varieties than could have
been anticipated. This is my view,
but what Hooker will say, when
he reads, as I hope he will, my
inquiries on the subject I know
not. Taking Babington as a guide,
I find in local Floras & in the
only 2 Entomological works, which
I have read, that the rule is
almost universal, that the larger
genera have more species with
varieties (& a greater average number of
varieties to the varying species)
than the smaller genera. —
You asked to see my sheets as
printed off; I assure you that
it will be the highest satisfaction
to me to do so: I look at the
request as a high compliment.

end to the necessary disquisitions. I have just finished a chapter on Instincts, & have found grappling with such a subject as Bees cells & comparing all my notes made during so years took up a considerable length of time.

But I am running on about myself in a most egotistical style. Yet I must first say how useful I have again & again found your letters, which I have lately been looking over & quoting; but you need not fear that I shall quote anything you would dislike, for I try to be very cautious on this head. — I most heartily hope

You may succeed in getting some "members" of old works off your hands & be in some degree a free man.

I especially have most kindly sent me his ~~Introductory~~ two first Part; but I confess I am disappointed: I cannot realise his views on the value of the higher groups, & all his speculation on a type.

Again let me say that I do indeed feel grateful to you & believe me

My dear Gray

Your most truly

C. Darwin

20

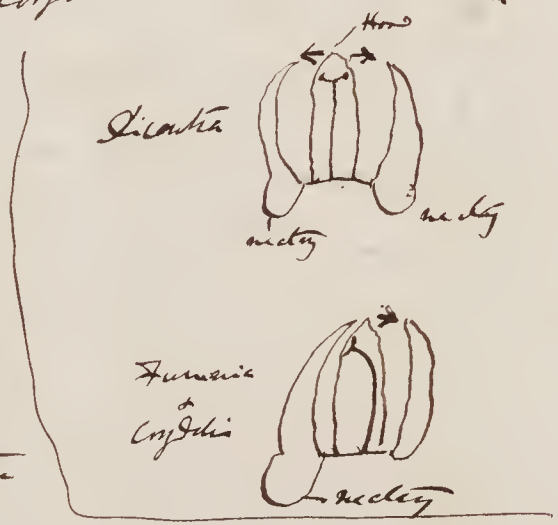
Down Bromley Kent

July 4th – 1858

My dear Gray

I have not answered your note of May 21 for I have had death & illness & misery amongst my children. And we are all going immediately from home for some weeks. With respect to Dicentra it is really pretty to watch the Humble-Bees sucking first on one & the other side of the several flowers: with their hind legs resting on the crest of the hood formed of the inner united petals they push it to opposite side of flower, & the straight pistil is rubbed against their abdomen & inner side of thighs, which are white with pollen from the several

flowers. It is impossible but that the individuals of Dicentra must be largely crossed. Your Adlumia has not flowered with me yet. In Fumaria & Corydalis we have another structure. viz. nectary on one side & here the pistil bends so that the 2 stigmas are presented to the gangway to the one nectary; & the hood slips off easily in opposite direction, instead of equally easily to either side. Indeed, in Corydalis unless it ~~only~~ is almost springs off, & the pistil decidedly springs towards the nectary = bearing petal. I have ~~to~~ observed only 6 Fumariaceae, & I wish the




I knew whether the rule was general; for I cannot believe that the structure of these flowers is related directly to the visits of Bees.

I suspect from my few observations that the following rule may be generalised (& I shd much like to know whether it is true) that where ~~there is no~~ honey is secreted on one point of circle of corolla, the pistil if it bends, always bends to that the stigmas, when mature, lie in the gangway to nectary. Thus in Columbine where there is a circle of nectaries, the stigmas are

straight; in Aquilegia grandiflora there are necteries on the stigmas and protrusion bent to the very Bee (as I this day saw) humbles are there in extracting the honey. —

It is very unlikely, but if I any chance you have my little sketch of my notions of "natural Selection" I would see whether it or my letter bears any date, I P. be very much obliged. Why I ask this, is as follows. Mr. Wallace who is now exploring New Guinea, has sent me an abstract of the same theory, with uncommon coincident even in expressions. And he could scarce have

heard a word of my views. He directed me to forward it to Lyell. — Lyell who is acquainted with my notions consulted with Hooker, (who read a long sketch a dozen years ago of mine written in 1844) urged me with much kindness not to let myself be quite forestalled & to allow them to publish with Wallace's paper an abstract of mine; & as the only very brief thing which I had written at was a copy of my letter to Gray, & ~~which~~ I sent it and it has just been read, I believe, (though never written, & not fit for such purpose) before the Linnean Soc.; & then in the season, why

I shd be glad of the date.
But do not hunt for it;
as I am sure it was
written in September, October
or November of last year. —
I have troubled you with a
long story on this head; to pray
forgive me & believe me
Yours very sincerely
C. Darwin. —

P.S.
In regard to bees visiting
nectaries, I shd say that I largely
judge of visiting of nectary by
seeing where Bees suck; according
to this, the rule holds in all
Leguminosæ, in Fumariaceæ, in
Iberis & Tropæolum — vide Trifolium, Dictamnus
Æsculus, Rhododendron — Aquilegia.

up the mountains, as they became denuded
of snow; & we now see on their
summits the remnant of a once
continuous plain & fauna. — This is
E. Faber's theory, which however
I may add I had written out
4 years before he published.
Some facts have made me suspect
that ~~after~~ between glacial & present
temperature there was a period
of slightly greater warmth. —
according to my modification =
= of theories. I look at many of
the species of N. America which
closely represent those of
Europe, as having become
modified since the pliocene

Aug. 11th (Down, Bromley Kent)

My dear Gray

Your note of July 27th has just reached
me in Isle of Wight. — It is a
real & great pleasure to me to
write to you about my notions; &
even if it were not 10 P.M. on
a most unexpected day, after all
the vicissitudes & fatigue which you
have endured, I should not
do anything which you asked.
I have discussed in long M.S. Ch.
the later changes of climate & effect on
migration, & I will here give you
an abstract of abstract, (which latter I
am preparing for my wife to work from
first. So! I cannot give facts, &
I must state dogmatically, though I
do not feel so on any point. I may

just mention in order) that you may
believe that I have some foundation for
my views, that Hooker has read my
M.S., & though he at first demurred
to my main point, he has since
told me that further reflection &
new facts have made him a
convert. — In the Miocene or perhaps newer
(a little before glacial epoch)
the temperature was higher: & there
was a little drift: the
land on a large scale held
much its present disposition:
the species were marine, judging
from shells, what they are now.
At this period, when the animals
& plants ranged 10° or 18° nearer
the poles, I believe the northern
part of S. Asia & of N. America
being almost continuous were
peopled (it is quite possible considering
shallow water the Behring strait
was united, perhaps a little subsided)
& a nearly uniform fauna &
flora, just on the arctic regions
now are. — The climate then
became gradually colder till
it became what it now is;
& then the temperate part of
Europe & America would be
separated, as far as migration
is concerned, just as they now
are. — Then came on the glacial
period, driving far south all
living things; & southern Europe being
peopled with arctic productions.
As the warmth returned
the arctic productions slowly retreated

Tropics & reached to southern temperate; & the few southern forms penetrated in tolerable direction northward (Height of Borneo with Australian forms, Abyssinia with Cape forms). Whereas there was many extensive high land, the migration would have been immensely facilitated: hence European character of plants of S. del Fuego & summit of Cordillera: hence ditto on Himalaya. As the temperature rose, also the temperate vegetation would climb up the mountains. Hence the European forms on Neilgherries, Ceylon, summit of Java, — Organ Mountains of Brazil. — But these vegetations being surrounded with new forms would be very liable to be improved & modified by natural selection to adapt them to the new forms with which they had to compete: hence most of the forms on the mountains of the Tropics are not identical but representative forms of temperate ~~European~~ plants. —

There are similar classes of facts in nation production, all these will appear very dull to you & such it may be; but I am sure not

so much as it did at first
appear to you; Hooker could not
stomach it all at first, but
has become larger a convert.
From Kamschatka & Shallow sea, I
believe Japan to have been
joined to mainland of China
within no remote period: & then
to migration N. & S. before during
& after glacial epoch would
act on Japan, as on corresponding
latitudes of China & U. States.
I ch. beyond anything like to
know whether you have any
Alpine collection for Japan
& what is their character.

period, also in northern part, then
we want free communication
between N & new worlds.
But we come a more important
consideration, there is considerable
body of geological evidence that
during glacial epoch, which would
we allow: I refer the many
years ago from erratic Boulder
phenomena carefully observed by me
on both E & W coast of S. America.
Now I am so bold as to believe
that at the height of glacial
epoch, & when all Tropical
productions must have been
considerably distributed, the
sea received temperate forms along
the belt into the heart of the

The letter is nicely expressed, but perhaps it will suffice to show that I believe has been the later main migrations & changes of temperature. —

With respect to our dispute on Ficariaceae, I am delighted to see that we have <u>absolutely</u> agreed; for I have supposed the structure of their flowers to do more than form an <u>occasional</u> exp. perhaps once in several generations. —

But how you attended to the point: plant a cottage garden, 12 hitherto varieties a table, moderately where each other, & the perpetuation of

Hangeline plants is unknown:
indeed sometimes hardly any
come from seed
raised under such circumstances.
I have counted perhaps. —
how the stigma of each
flower is surrounded not only of its own
stamens, with pollen shed on
top a flower, or, but of
a multitude of other flowers
with pollen of the same variety;
& yet this hangeling takes
place to an enormous extent.
I believe a cross is so
beneficial, that the pollen
of a distinct variety
has a peculiar action

over the plants' own pollen.
You will see it & inference
which I (?) draw in regard
to Fertilisation. —

Whenever my abstract is published
next winter, I will of
course send you a copy;
though by the way I shall
not give abstract on
facts in regard to crossing,
for they are too many
to abstract. —

I fear the the letter L
my horrid bad handwriting
will be. Write you to

read Belian in

by dear Fry

from most sincerely

C. Darwin

yourself to death

Pray believe me
My dear Gray
Your most sincere
C. Darwin

A sort of vague feeling comes over me that I have asked you all this before: if I have, I beg very many apologies. — I know I am worth several letters to earn my part of world for similar information.

Down Bromley Kent

Nov. 18th

My dear Gray

It is a horrid shame to trouble you, busy as you always are, but there is one point on which I am very anxious to gain information & perhaps it may be gained in the S. of your country & I can think of no one to apply to but you. Old writers often insist on differences of constitution going with complexion; & I want much to know whether there is any truth in this.

It has occurred to me that
liability to such a disease
as yellow-fever would answer
my question in a back
possible way. Do you
know anyone of a scientific
mind to whom to apply
to ask whether any
observations have ever been
made or published, whether
Europeans (either of course
any crossed with negro-
blood) of dark complexion &
black hair are more liable
or less liable to be attacked
(or any other form)
with yellow-fever, than
persons of light complexion.
If you could aid me in
this it would be of much
value to me. But do not
trouble yourself to write merely
to acknowledge this. —
I have just published a little
notice in Gardeners Ch. on
the fertilization of Leguminous
plants, which rather bears
on an Fumariaceous discussion.
I sincerely hope that you
are well & not working

1859

Down Bromley Kent

Nov. 11th

My dear Gray

I have directed a copy of my Book (an set off an abstract) on the Origin of species to be sent you. — I know how you are pressed for time; but if you can read it, I shall be infinitely gratified. From its condensed state it is indispensable to read it the straight through. If ever you do read it, & can screw out time to send me (as I value your opinion so highly) however short a note, telling me what

your view

his & your view

rather dull whether this will explain facts of distribution so well as my view of migration during the certainly warmer period anteriorly to the glacial period.

Anything on my heterodox doctrine of modification ~~&~~ your view does not bear on the representative species in the middle U. States. I thought of your doctrine several years ago, & consulted Lyell, but

you think its weakest & best parts, I shall be extremely gratified. As you are not a geologist you will excuse my conceit in telling you that I feel highly approve of the two geological chapters; & think that on the Imperfection of the Geological Record not exaggerated. He is nearly a convert to my views.—

I have been so hard worked & my health so bad that I have not yet read your Japan Flora; but I will hereafter borrow Hooker's copy. Hooker sent me a few pages in which you propound a doctrine of migration into America like that, which I sent you last summer in a letter. Dana is of course a far better authority than I am; but his arguments have by no means convinced me in regard to the warm period subsequent to the Glacial period. I dare say I may be wrong, but I

Your Japan work must have been extremely interesting.

Farewell. With every good wish, especially for a little peace for you to stay between us

Dear Gray

Your very sincere

Charles Darwin

P.S. I suppose that you have not succeeded in getting any information as to correlation of complexion in Europeans & tendency to fellow at the Tropical Fevers.—

Let me add that I fully admit that there are very many difficulties not satisfactorily explained by theory of descent with modification, but I cannot possibly believe that a false theory would explain so many classes of facts, & I think it certainly does explain.— On these grounds I hope you & believe that the difficulties will slowly disappear.—

regarded it as less safe than
to warm period intervening
to glacial period. There
seemed to me some little
confusion about some fossil
elephants: the species in Mr.
J. Slater, I believe, an
Indian according to Falconer.
The northern one, anyhow,
can hardly be adduced as
evidence of warmer climate.
But it is foolish in me
to write on; for I am
writing up after one has
read, recent matters &c. —

I will have the clean
sheets sent over in
a few days of as many
sheets as are printed
off & the remainder afterwards,
& you can do anything
you like. — If nothing
then is to have done,
I shall be glad for
the new Edit to be
reprinted, & not the old. —
My great hearty & warm
hearty thanks.

Pray my sincerely
C. Darwin

I will write 100k again



Down Bromley Kent
Dec. 21

My dear Gray

I have just received your
most kind, long & valuable
letter. — I will write again
in a few days, for I am
at present unwell & much
pressed with business. — To
day only a very personal.
I shall for several reasons
be very glad of an
American Edition; I have
made up my mind to be
well abused; but I think
it of importance that my

notions to be read by intelligent men, accustomed to scientific arguments though not naturalists.

It may seem absurd but I think such men with drag after them the naturalists, who have too firmly fixed in their heads that a species is an entity. — The 1st Edit of 1250 copies was sold on first day, & now my Publisher is printing off as rapidly as possible 3000 more copies. — I mention this fact because it renders possible a remunerative sale in America. — I shd be infinitely obliged if you could aid an American Reprint; & could make, for my sake & Publisher's, any arrangement for any profit. — The new Edit. & any Reprint, for which I have made a few important corrections.

Down Bromley Kent

Dec. 24th

My dear Gray

I have been for 10 weeks at Water Cure & on my return a fortnight ago there I found a copy of your memoir, & heartily do I thank you for it. I have not read it, & shall not be able for some days, for I am much overwhelmed & my stomach has ~~so little~~ got nearly as bad as ever.

With respect to the question of climate, I beg you to believe that I never put myself for a moment in competition with Dana; but when one has thought on a subject, one cannot avoid forming some opinion. What I wrote to Hooker I forget, after reading only a few sheets of your memoir, which I saw would be of the highest interest to me. Hooker asked me to write a page or opinion, but as I told him, I could not presume to do so, but on reflection, without careful deliberation, I have not. I had pondered several years ago what he wrote (& when I forget) some speculation on warmer period in U. States subsequent to glacial period; & I had consulted Agassiz who seemed much to

Journey write be sent to Boston for Dec. 25 & have been kept off the sheets, as must be in last practicable.

doubt, & Hell's pigment is really admirably
cautious. — The arguments adduced in your
paper & in your letter seem to me hardly
sufficient; not that I ~~do~~ be at all
this subsequent & intercalated warmer period
willing to admit, — the more changes the
merrier I think. — On the other hand
I do not believe that introduction of
Old world forms into New world ~~[illegible]~~ subsequently
period
to Glacial, with ~~[illegible]~~ & for
the modified or representative forms
in the Two Worlds, there has been too much
(the little change of the isolated)
change in comparison with Alpine
forms: but you will see this in my
Book. — I may just make a few remarks,
only at first sight I do not attach much
weight to the argument in your letter about
the warmer climate. Firstly about to back of the
land being been lower subsequently to Glacial
period, as evidenced by the whole &c. I doubt
whether meteorological knowledge is sufficient for this
deduction: turning to the S. hemisphere it might
be argued that a greater extent of water made
the temperature lower. And when much of the
northern land was lower, it would have been easier
for the sea & intermigration between Old & New
worlds would have been checked. Secondly I doubt
whether any inference as return of climate
can be deduced from extinct species of
mammals. If the musk-ox & deer of
great size of your Barren Grounds had
been known only by fossil bones, who
would have ventured to imagine the sleeping
cold climate they lived under. With
respect to food of large animals, if
you care about the subject with your turn to
perhaps in respect to the Elephas primigenius
my discussion on this subject in my
Journal of Researches (Murray's Home & Colonial
Library) Ch V. p. 85. — In this country we
infer from remains of Elephas primigenius,

that the climate at the period of the
embedment was very severe, as seems countenanced
by the worldly coloring of the nature of the
with angular fragments counted in
deposits — the nature of the shells, & evidence
of the Mark Ox. I had formerly gathered
from Lyell that the relative position
of Megatherium & Mylodon with respect
to the glacial deposits, had not been
well made out; but perhaps it has
been so recently. — Such are my
reasons for not as yet admitting the
warmer period subsequent to glacial
epoch: but I dare say I may be
quite wrong, & shall not be at
all sorry to be proved so. —
I shall eagerly read your Essay with
care, for I have seen no sets of a
fragment: & very likely some parts, which
I could not formerly clearly understand, will be

clear enough. — And I am very sorry the Hooker said a word about my opinions. —

I have been much interested & struck that you say in early part of your letter about "Creation" & the philosophy of the subject. As you truly remark, if we admit a certain amount of Creation there is no strong reason & why not admit many acts. — But I look on the fact that the theory of Natural selection explains many classes of facts, which, as far as we can see, repeated acts of Creation do not explain. On this latter view we can only say "so it is" & not as "why it is so". — Pray do not allow either way to decide, the you have read Ch. XIII &

(CLXIV)

the Recapitulation, which will, I think,
aid you in belonging facts. — I value
your opinion much highly; & I hope
you will think me presumptuous
in saying how sincerely I have admired
your several letters to me & Mr to
Hooker which I have occasionally
read. I am rejoiced to say that
Fritz is a complete convert; & is logic
& candid enough to be now writing a
public change of opinion. — Huxley, a
first-rate zoologist, & Carpenter first-rate
physiologist are convert; as is H. C. Watson.
So that I am again thus contented. I
am sure to be in error in many parts;
but my general view, I conclude, must have
some truth in it. — Now are many bitter opponents.
I send to copy of my Book though missing. I had written at
the Forbesian doctrine of Alpine plant 4 years before Forbes
published, as Hooker knew, but I do not ~~suppose~~ believe the Forbes
had ever heard of it: so he is independent, as you will see in my
Book. I do not allude to my first work in MS. My dear Sir
your most sincerely
C. Darwin

to understand your geology
at p 447, 448; but you
would probably not care
to hear my difficulties &
therefore I will not
trouble you with them.
I was so grieved to get a
letter from Dana at Florence,
giving me a very poor (though
improved) account of his health.
Believe me, my dear Gray
Your very sincerely
C. Darwin

What an admirable memoir
on the distribution of American
plants is that of Hooker!

Down Bromley Kent
Jan 7th

My dear Gray
I have just finished your Japan
memoir & I must thank
you for the extreme interest
with which I have read it.
It seems to me a most curious
case of distribution & how very
well you argue & put the
case for analogy on the
high probability of single
centres of creation. That a
great man Agassiz, should
be wrong to reason seems to
me as great in taking
a wrong view as he is great

in observing & deploring. One of the points which has struck me as most remarkable & inexplicable in your memoir is the number of monstrosities (or near so) given amongst the representative forms of Japan & N. America. And the very singular preponderance of identical & representative species in Eastern compared with Western America. — I have two good maps showing how wide the modestly low country is on the west side of the Rocky Mountains: most of course do I know whether the whole of the low western territory has been determined; but it has occurred to me looking at such maps as I have, that the Eastern area must be larger than western which would account for to certain small extent for preponderance on Eastern side of the representative species. Is there any truth in this surmise? Your memoir sets one marvelling & reflecting. — I confess I am not alone quite

Down Bromley Kent
Jan. 28th

My dear Gray

Hooker has forwarded to me your letter to him; & I cannot express how deeply it has gratified me. To receive the approval of a man, whom one has long most sincerely respected, & whose judgment & knowledge are universally admitted, is the highest reward an author can possibly wish for; & I thank you heartily for your most kind expressions. —

I have been absent from home for a few days, & so could not earlier answer your letter to me of

or additions of equally small importance, or letter of equal brevity. — I, also, intend to write a short Preface with a brief history of subject. — Then I will set about, as they must now day be done & I will send them you in a short time, — the few corrections first, & the preface afterwards, only I hear that you have given up all idea of separate Edition. You will then be able to judge whether it is worth having new Edition with your Review prefixed. Whatever be the nature of your Review, I shall be proud to have my Book thus preceded. — In business matters it is always best to speak out

the 10th of Jan.y — You have been so very kind to take so much trouble & interest about the Edition. It has been a mistake of my publisher not thinking of sending over the sheets. I had entirely & utterly forgotten your offer of receiving the sheets as printed off. But I must not blame my publisher; for because ~~[illegible]~~ had I thought= =been your most kind offer, I feel pretty sure I shd not have taken advantage of it; for I never dreamed of my Book being so successful with general readers: I believe I shd have laughed at the idea of sending the sheets to America. —

After much consideration & on the strong advice of Hall & others, I have resolved to have the present book as it is (excepting correcting errors or here & there inserting short sentences) & to use all my strength which is but little to bring out the first part (forming a separate volume with index &c) of the other volumes which will make my bigger work; so that I am very unwilling to take up time in making corrections for an American Edition. — I enclose list of few corrections in the 2d Reprint, which you will have received by this time complete; & I could send 4 or 5 corrections

plainly: as I should not, as you have managed it, get anything from an American publisher, if you do print an Edition & can get any profit, nothing should induce me to touch a penny of it. My terms with Murray are that I receive 2/3 of profits, & he 1/3; — with respect to latter, I can of course say nothing. — But I expect when you come to consider the case, you will not think a new Edition worth thinking about; though an answer to Longfellow would be a <u>great</u> advantage to subject. —

Jrey 43

Thank you much for telling me
the magnificent compliment of
Wyman & for sending me extract
from his letter. I cannot see the
force of his argument; & if he
wished to profit by book he
could not have been more
ingenious. — I am delighted
to hear that Mr. D. Rogers, the
Professor at Glasgow & a gallant & philosophic
long way with my views. —
Believe me, my dear Gray
I feel all your most
generous sympathy & assistance
Yours most truly
C. Darwin.

I shall value much at any time your
criticisms, either in your Review or by letter.

44

Down Bromley Kent
Feb 1

My dear Gray

I received yesterday your letter of the 17th. I am heartily glad you have so kindly determined to reprint; but I especially hope that your Review will appear in it. — The Edition might be entitled "Reprinted from corrected Second Edition with additional corrections". I endeavor to do some few additions (all that I have) & will send in a fortnight a Preface giving a short history of opinion on Origin of species. —

Since writing last I have had a letter from Murray, the publisher; & he says "whatever benefit may arise from the Reprint of your Book in U.S. you are fully entitled to". — So he claims nothing. Perhaps you would like me to stand in Murray's position & take 1/3 of the profits whatever they may be. — Nothing would pain me so much as to take all the profit, as I shall & absolutely shall owe all to you; & if you with print your Review, it will be a joint publication.

Believe me, My dear Gray
Your most truly & gratefully
C. Darwin

(The article in Times & Huxley. —)

If my additions & Preface should arrive too late, will not be labour in vain, as they will be for Fourth Edition [illegible] Q. Review. —

Down Bromley Kent +

My dear Gray

I now send my short Hybrid Species & can have none of contractions. With respect to Agapeg, I have throughout followed but Bentham is fully as good an authority; some would say better authority. Please forward enclosed to Agapeg & read if you think with advice.

I write all the last part of column from memory, & it is a blessing mine thousands can not get, have not been detected; but the state of my health left me no choice; I felt on the point of gentle breaking down. If the M.S. (as before) is of no use, no harm is done. The pages also refer to the new Edition or Reprint.

I have now received your letter of the 23d of June & Hooker, in which you told me all to trouble (& you so much overwatched) which you have taken about an American Edition. I fear there is no chance of your Revision not appearing at the heat, which would have greatly pleased me. If there is another Reprint in America, with you sent the English 2d Edit (or Reprint) on account of several verbal corrections & the M.S. I would send you another copy, or make the likewise corrections Publishers give you one. If you notice any errors like "self-fertilise itself" pray correct. Again & again, I thank you heartily for all your generous kindness to me. Now I write just two things some points in your letter. What you say about my Book gratifies me most deeply & I wish I could feel all was deserved by me.

(Letter continued)

I quite think a Review from a man, who is not an entire convert, if fair & moderately favourable, is in all respects the best kind of Review. What weak points I agree. The age to this day gives me a cold shudder, but when I think of the fine known quickening, my reason tells me I ought to conquer the cold shudder. I agree about sterility, & especially about fertility of the strongest marked varieties, & in the M.S. already sent to you, I have confessed more plainly the difficulty. But a vast number of facts show how mysteriously & easily the reproductive system is affected. Pray kindly remember & tell Prof Wyman how very great I do be for any hints, information or criticisms I have the highest respect for his opinion. I am so sorry about Dancy's health; I have already asked him to beg you a visit. Farewell, you have laid me under a load of obligation — not that I feel it a load. It is a highest possible gratification to me to think that you have found my Book worth reading & reflecting on. Pray give my thanks to the judge whose opinions I shall value most of all.

My dear Sir
Yours most sincerely
C. Darwin

[illegible]

37

New York Feb 17/60

Dear Sir

Yr favor of 15th is at hand. We cant say what we can do respecting the notes & additions till we see them but we shall be anxious to make our edition conform to any future English Edn. -

You are under a mistake in supposing that new matter unpublished in England secures a copyright in this country if written by Mr. Darwin - on the contrary if the entire work had never been published in England and first appeared here no copyright would hold in this country as no one can hold a copyright here for what he has written unless he be a citizen of the U.S. -

Prof Asa Gray

We

We proposed to ourselves to pay 5% on retail price as suggested in yr letter, as there is no reason why a work without any legal rights, shd pay the same as one that is secured by law – We desire to act liberally altho' we printed the work after ~~the book~~ it had reached this country some little time; not having rec'd even early sheets which is usual when any payments are made. We regret very much there is no protection to the foreign author, think it a monstrous shame, but we are obliged to take things as they actually exist – We are quite willing if it wd be agreeable to Mr Darwin to send him a check for 50£ stg. and very likely that will be as much as he could receive by the sales.

We remain
Very respectfully
D Appleton & Co

Feb. 20. 1860

My Dear Darwin.

I get thus to-day I send Appletons, now, the sheets of ed. 2. & your additions appended in their places, I promise the Historical Preface next week, and I put it in their hands – trusting to their promise of 5 pent and to their honor for more if they are not molested by reprints, which we shall keep off.

The offer of check for £50 –

(which I might send to Mrs. Darwin for pin-money, since you seem it) tempts me, - but I think it wise to wait and hope for wise.

I mailed you a clean copy of my Review. I have sent it to Agassiz. He is childishly apt to be offended at any opposition, but I have, as you see, been very careful to avoid all cause of personal offence.

Ever, Dear Darwin,

Yours cordially

Asa Gray

Down Bromley Kent

Feb. 18th

My dear Gray

I received about a week ago two sheets of your Review; read them, & sent them to Hooker; they are now returned & I retain with me, & tomorrow I send them to ~~Hooker~~ Bell. —

Your Review seems to me admirable; & far to be the best thing I have read. — I thank you from my heart ~~also~~ both for myself, but far more for subject — rather. How curious your criticism

theory & explaining an ample lot of facts; again & again I thank you for your generous aid in discussing a view, about which you very properly left yourself unbiased. —

My dear Gray
Your most sincere
C. Darwin

Then — Review in last Athenæum is very of L. H. on my Book — rather bitter: I feel much certainty it is of my good friend the Entomologist Wollaston: I have not detected any new idea for this Review. —

Second Clergyman so far with me. — Rev L. Jenyns, a very good naturalist: then two with so a very

[illegible] I wish I knew who it was: I should be pleased to see it.

between the views of
Agassiz & such as mine
in my criticism & criticism.
By the way of Agassiz
writes anything on
subject. I hope you will
tell me — I am
charmed with your
metaphor of the theatre.
never thought against
the force of gravitation.
Your distinction between
an hypothesis & theory
seem to me very disagreeable;
but I do not think it is
ever followed. — Everyone now
speaks of the undulatory theory
of light; yet the ether is
itself hypothetical & the
undulations are inferred only
from explaining the phenomena
of light. — Even as the
theory of gravitation, is the
attraction given in any
way known, except by explaining
the fall of the apple & the
movements of the Planets?
It seems to me that an
hypothesis is developed into a

that for an Omnipotent & Omniscient Creator to foresee is the same as to ordain; but then when I come to think over this I get into an uncomfortable puzzle <u>something</u> analogous with "necessity & free-will" or the "Origin of Evil", or other such subject quite beyond the scope of the human intellect. I was interested the other day in reading the Life of Hunter & Brunton, to find that Leibnitz actually attacked the Law of Gravity as "subversive of all natural Religion"!! He further attacked Newton for having

Down Bromley Kent

Feb 24.

My dear Gray

I received yesterday your kind letter & the remaining sheets of the Review, which I forwarded to Hooker, to go to Dr Bolle & then to Bell. — This remainder of the Review most fully confirms my opinion that it is far the most able which has appeared, & you will have done the subject infinite service. And now I most sincerely hope that it will cost you no more labour. —

Hell quite agreed with me
that the two first chapters were
the best thing which has
appeared on the subject. —

Thank you much for your letter,
which as it contains such
curious botanical facts, I
have lent to Hooker. Why
should you call me of the
best essays ever written (at
least in my opinion) on Geograph.
Distribution of Plants "unfortunate".
I really hope, as a punishment
to myself, that I may be found
equipping away about to wander
pleasant subsequent to the glacial
epoch. — Certainly very much more
has to be made out about the
latest geological periods. —

I am very glad to think (if
I understand rightly, but I must
reread your letter) that the
greater resemblance of Mid American
floras of L. W. America to Asian
in the plants, is not quite
so great an anomaly as it
at first appears. —

I have been interested by your
theological remarks in the Review,
but I must reconsider them.
It has always seemed ~~[illegible]~~ to me

used gravity "an occult quality" to explain the motions of the planets. — Newton answered that it is philosophy to explain movement of wheels of clock, though the cause of descent of the weight could not be explained. This seems to me better to bear on what you say of heat. Selection not being proved a vera causa. —

I cannot but think you lay rather too much stress on new organs appearing: in the animal kingdom, at least, it is most difficult

to show any number of new
again. — Mr Edwards has
tried with my little
success. The 1st were
forgot such transitions
as at least such gradations,
as a swim-bladder into a
lung. — So even with
the Eye, as numerous fine
gradations can be shown
to exist, to perfecting the
wonderous organ of Nat.
Selection. I must look
at as a difficulty to
our imagination & not to

our reason. — In the U.S.
edition sent you for my
future edition, there is
one blundering sentence,
however corrected. If I
knew there wd be a
second American Edition
I would send a few
more additions & corrections.
With most sincere thanks
for all your kindness.
Believe me
Yours most sincerely
Chas Darwin

14

Brow. March 2d

My dear Fry

I have had my shot at the hardship
lower of illness, — a slight touch of
pleurisy, & one week. — but
write just to thank much for
letter received today (some two
endorsed letters have been posted)
& to say that in the week
I hope & think I shall send
you a few more letters
important additions & expansions
for the "Origin". The first & most
considerable one is for p. 126.
If you can anyhow manage

to get them inserted I
shall be very glad — It is
of some page length. — There
are a few of the most
objectionable parts later
& should be expanded a little.
I am tired. so no more

C. Darwin

61

[illegible] Bromley Kent

March 8th –

My dear Gray

I now send some more M.S. I sh. be extremely glad if they are not too late for the Edition. If the long addition for p. 126 is too late, that at p. 336 cannot be inserted, as they hang together – unless indeed the M.S for p. 126 were given as a supplement. These additions seem to me, judging from number of letters which I have received on the subject of some little importance. – I will, for future, keep to my resolution of not wasting more time on the origin. So that you need not fear my giving more trouble. Most sincerely do I thank you from my heart for all your generous kindness & interest about my book. If there be any profit I am not so foolish as to despise it, but you are perfectly right – that I can more for a good & corrected American edition; & it is the simple truth that I sh. be infinitely pleased to share profits with you; & this would be fair in many ways. Your truly admirable Review would make a most useful preface, so I hope the Publishers will agree. Believe me that I fully appreciate your motives in taking such great trouble about my Book. – I have not yet been able to send copy a second time all your Reviews; I have had to lend it to so many persons. – I have had a long letter from Dr. Booth, full of the most noble love of truth & candour. He goes far with me, but cannot swallow all. No one could until he had enlarged his gullet & sharpened his practice, as in my own case. – You ask about Jonathan Da

I am his grandson. — How extraordinary is the state of mind of Agassiz: it is, as you say, a case of "sheer madness mad." Owen after much shuffling & secrecy; with bitter sneers to some & modified praise to others, has just spoken out that he rejects my views on the ground of the imperfection of the geological record. But this is just the subject on which he knows nothing; for in his life he has never examined a single stratum. I can count in England 4 geological concerts & 8 or 9 other naturalists. I hear the Thwaites of Ceylon is one. — Did you see Harvey in Gr. Chronicle versus the Origin: the article, I thought, rather weak; & Hooker's answer admirably good. I send Macmillan, not that it is with sending: look at the verses at the end of following article! Farewell my kind friend. Believe me, yours most sincerely

C. Darwin

If possible I should like out of curiosity to receive through Murray or Albemarle St a copy of the American Edition —

P.S. Since the above was written I have received Appleton's letter to you with his P.S. — I much fear that the additions herewith sent will be too late.

You might tell the Appletons that I mean to have all future Editions of the "Origin" (as far as I can tell) in its present state with the additions now sent. —

Heartily thanks about the money. I should have thought a bird in Hand worth two in the Bush. — The £50 my handsome. — With renewed thanks

Ever yours

C. D.

31

of your Cambridge & he brought
me direct from Leipzig all
sorts of very civil speeches.
What can the meaning? I hope
to God he is a sincere
man; I had always fan-
-cied that he was so —
You may like to hear
about Reviews on my Book.
Sedgwick (as I & Geek feel
certain from internal evidence)
has reviewed me largely
& unfairly in the Spectator.
The notice includes much
abuse & is hardly just
in several respects. He
would actually lead anyone, who

[4] posted letter to Vikarsia

Down Bromley Kent
Apr. 3d [1860]

My dear Gray

Although I have nothing particular
to say I must thank you
for your pleasant letter of
March 19th. — But let me say
I know what a busy man you
are, & pray do not waste
more time over me. My book,
your Review, & letters &c &c must
have consumed an awful
amount. In one sense the time
spent on Review has not been
wasted; for I feel sure (& I
have again (3d time) read it also concurring)
that it will produce great
effect in leading people to think,

& that is all I wish. Under indeed
letters may be known cases where
your article has had this effect
& has greatly modified opposition
to my Book. I agree largely
to what you say about "vera causa"
"theory", "hypothesis": indeed on
reading your whole review in
the Nat I saw that some
of my remarks were rather superfluous.
It is curious that I remember well
time when the thought of
the eye made me cold all
over, but I have got over
that stage of the complaint,
& now such trifling particulars
of structure
& often make me my uncomfor=
=table. The sight of a feather
in a peacock's tail, whenever
I gaze at it, makes me
sick! Under this point of
view your story of the Black
Pigs in the Everglades delight=
=ed me, & supports other cases, which
though founded on very good
evidence I could hardly
digest. — Pray keep Prof. Ogmen
up to the mark about writing
to me; I shd, also, look on
it as a great honour & favour.
If you happily can find out
hand positively the red
nuts. — I think be very
curious to see Agassiz's remarks:
I met a few days ago Prof. Cooke

think there was a good chance of my becoming the most egotistical man in the country! What a proud preeminence! — With you have helped to make me so, & therefore you must forgive me if you can. —

My dear Gray

Ever yours most gratefully

C. Darwin

Dec 4/7

+

was ignorant of geology, to suppose that I had invented the great gaps between successive geological formations; instead of its being an almost universally admitted dogma. But my dear old friend Sedgwick with his noble heart is old & is rabid with indignation. — It is hard to please everyone: you may remember that in my last letter I asked you to have out about the demolition of the World: I told Jukes this (who is head man of Irish geological survey) & he blamed

too much for be believed
everyward of it, & thought it
not at all exaggerated!
The fact notwithstanding has no
means of gauging the
infinitudes of last time.
There has been one prodigy
of a Review, namely an
<u>opposed</u> one of Pictet
the palaeontologist in the
Bib. Univers. of Geneva,
which is <u>perfectly</u> fair &
just & I agree to every
word he says; one of difference
being that he attaches less
weight to argument in favour
& more to argument opposed,
than I do. Of all the
opposed reviews I think this
the only quite fair one, &
I never expected to see
one. Please observe that
I do not class your
Review of my memoir as
opposed, though you think
so yourself! it has done
me <u>much</u> too good service
to appear in
that light in my eyes.
But I fear I shall vex
you with so much about
my Book. I shall letter

a note, which I have seen, full of strongest praise! What a strange man he is. All say his malignity is merely envy because my Book has made a little noise. How strange it is that he can be envious about a naturalist, like myself, immeasurably his inferior! But it has caused me a good deal to be treated thus by a friend of 25 years standing. He might have been just as severe without being so spiteful. — Owen considers himself by saying this to whole subject will be forgotten in ten years. —



Down Bromley Kent
Oct. 25th

My dear Gray

I have no doubt I have to thank you for copy of a Review on the "Origin" in N. American Review. It seems to me clever, & I do not doubt will do damage to Book. I had meant to have made some remarks on it; but Lyell wished much to keep it, & my head is quite confused between so many Reviews which I have lately read. I am sure the Reviewer is wrong about Bees cells, i.e. that a distinct organ before distinct would do, or even greater distinct, but then some of the glands would be which to generate spleen, but then would not add much difficulty to work — The Reviewer takes a strange view of

instinct: he seems to regard intelligence as developed instinct: which I believe is wholly false. — I suspect he has never much attended to instinct & minds of animals, except perhaps of reading. My chief object is to ask you if you could procure for me a copy of the New York Times for Wednesday March 28th: it contains a very striking review of my Book, which I shd much like to keep. How curious that (i.e. you & this) two most striking Reviews shd have appeared in America! This review is not really unfair, but somehow is unpleasant. — There was good Review in the Revue des Deux Mondes, April 1st of Mr. Lanzet, said to be a very clever man. — Have you seen how I have been thrashed of Owen in last Edinburgh: he misquotes & misrepresents me badly, & has he lands himself. — But the manner in which he sneers at Huxley is scandalous, — to speak of his Essay & never allude to his work on Geological Distribution is scandalous. When Huxley's Essay appeared Owen wrote

Nov. 13

Have you noticed how completely I have been anticipated by a Mr. P. Matthew, in Gardeners Chronicle.

Hooker about a fortnight ago stayed here a few days & was very pleasant; but I think he overworks himself. He is beginning to experimentize on various subjects at Kew, & if he can get from time I think he will be glad of it. He is going to try to degenerate our culinary vegetables. What a gigantic undertaking, I imagine, his

& Benthams Genera Plantarum
with her! I hope he
will not get too
much wearied in it,
or not
to spare time
for Geography. Distances &
other such questions.
I have begun to work
steadily, but my head is
weak, & details or
variation under dictation.
My dear Gray
Yours always truly & gratefully
C. Darwin

a fight by best. You seem to have done so already in great style. And I believe Hooker will, as certainly will Tyndall & Huxley. But it will be a long fight. By myself I shd be powerless. I feel my weak health acutely, as I cannot work hard. — So bitter is the feeling of some that Hooker tells me the neither Harvey, Balfour, or Acosta have ever read his Australian Essay! Is this not incredibly petty? It makes me savage to



Down Bromley Kent
May 18th

My dear Gray

I thank you for your pleasant letter received this morning. I return Mr. Appleton's letter & you refer to two letters from them. I have habit of filing everything, & have looked through my whole file & can find no letter. It is provoking that he does not publish Mr. Ellis. & how kind it is in you to go on troubling yourself on the subject. — It seems very true that as you have put your hand in it, it is not so easy to draw it out. What a battle you do seem

to have been fighting on the
origin of species. – I can
most plainly see that whatever
amount of truth my book may
contain, the setting of it
(as I told Hooker the other
day) will indeed be wholly
due to a very small body
of men. Had it not been
for Huxley, Hooker, yourself & two
or three others, I am well
convinced my book & the
subject would have been
more flank in the pan. For
the attacks are now been
incessant & very bitter. For
instance Sedgwick & Prof.
Clarke attacked me savagely
at Cambridge Phil. Soc. but dear
old Henslow (though he goes but
little out with me) stood
up manfully for the subject
as legitimately within bounds of
science, & produced excellent
effect. – Since then Prof.
Phillips has delivered against
me at Cambridge – A.
Murray has read paper at
Royal Soc. of Edinburgh against me.
Dr Dawson in Canada has
mag. &c. Power on in Dublin
Nat. Mag. Mr Hampton
at Dublin Geog. Soc.
and others I could go on
for many more !!
But the effect on me is
that I will buckle on my
armour &

think of the slighting way in which Owen alludes to Huxley's Essay in Edinburgh Review. Spell is working my hand at geological history of Man; & the is really marvellous how rapidly minds fast are turning up. I expect his Essay, in which he will discuss origin of species, will make a great commotion with the reading public.

Thanks about red "roots". — I must sincerely hope that you my public you

Stevens of an answer to
Bowen against & Co. —
With hearty thanks

Yours most sincerely

C. Darwin

My dear Gray.

Down Bromley Kent

May 22d 1860

Again I have to thank you for one (of May 7th) of your very pleasant letters, enclosing a very pleasant remittance of 22 £. — I am in single truth astonished at all to find that you have letters for me. I return Appleton's account. — For the chance of your writing for a formal acknowledgment, I send one. — If you have any further communication to Appleton, pray express my acknowledgment for his generosity; for it is generosity in my opinion. I am not at all inspired at sale diminishing: my opinion inspires in at great rate of sale. Our trade to public has been shamefully misguided as! for they bought the book, thinking that it would be nice easy reading. I expect the sale to stop soon in England: Get Bell wrote to me the other day that calling at Newnap he heard that 50 copies had gone in previous 48 hours. — I am extremely glad that you will write a few additions in the Origin. Judging from letters (& I have just seen one from Thwaites to Hooker) & remarks, the most serious objection in my book was not explaining how it is, on I believe, the old from & not necessarily advance, — but there can be no visible organisation this existing. — The Review article in Med. & Chirurg. Review is I believe Carpenter. — I would send Pictet's, if you cannot see it, but I thd. require

it back. — Sedgwick has been living boarding with us, but returning on Saturday week. — Prof. Clarke of Cambridge says publicly that the chief characteristic of such books as mine is their "consummate impudence". — I hear there is very severe review on Mr D in North British by a Revd Mr Dennis, a free-kirk minister & dabbler in Nat. Hist. I shd be very glad to see any good American Reviews, — as they are also more or less useful. — You say that you shall touch on other Reviews. — Huxley told me some time ago that after a time he would write review on the Reviews, whether he will I know not. — If you allude to Edinburgh, they have been [illegible] of the point which I will point out on separate slip. In "Saturday Review" (one of the cleverest periodicals) of Aug 5th p 573 there is a nice article on Owen's Review, defending Huxley. He has Huxley; & the latter I think Owen treats most ungenerously. — But surely you will get sick unto death of me & my Reviewers. —

With respect to the theological view of the question; there is always possible to me. — I am bewildered. — I had no intention to write atheistically. But I own that I cannot see, in plain as others do, & as I shd wish to do, evidence of design & beneficence on the side of us. There seems to me too much misery in the world. I cannot persuade myself that a beneficent & omnipotent God would have designedly created the Ichneumonidae with the express intention of their feeding within the living bodies of caterpillars, or that a cat should play with mice. Not believing this, I see no

necessity in the belief that to you we expressly designed. On the other hand I cannot conceive to myself intellect to view the wonderful universe & especially the nature of man, & to conclude that everything is the result of brute force. I am inclined to look at everything as resulting from designed laws, with the details, whether good or bad, left to the working of what we may call chance. Not that this notion at all satisfies me. I feel most deeply that the whole subject is too profound for the human intellect. A dog might as well speculate on the mind of Newton. — Let each man hope & believe what he can. —

Certainly I agree with you that my views are not at all necessarily atheistical. The lightning kills a man, whether a good one or a bad one, owing to the exceedingly complex action of natural laws, — a child (who may turn out an idiot) is born & the action of even more complex laws, — and I can see no reason why a man, or other animal, may not have been abnormally produced by other laws; & that all these laws may have been expressly designed by an omniscient creator, who foresaw every future event & consequence. But to me I think the universe is too wonderful & beautiful, I believe; as indeed I have publicly stated of this letter. Most deeply do I feel your generous kindness & interest. — Yours sincerely & cordially

Charles Darwin

Asa Gray 37 for Mr. Darwin (26)
" Darwin "

# Statement of the Sale of Origin of Species to May 1st, 1860

| | | | | |
|---|---|---|---|---|
| On hand last account, | — | On hand this date, | 250 | |
| Printed since 1st/60 | 1500 | In hands of Booksellers, | 300 | 550 |
| Feby – 1/60 | 500 | Given away, | | 200 |
| March 1/60 | 500 | Sold to date, | | 1750 |
| | 2500 | | | 2500 |

1750 Sold, at 5% on 1.25 Copyright amounting to $109.37
= £22,00

July 5/a . Edinburgh Review

p. 489. I think I told you of the illiberality of
Owen about to Brandiscia; he having in inde=
=pendent works fully admitted that the Brandiscia
were Brandiscia

496 What a quibble to pretend he did not understand
what I meant by <u>substitutes</u> of S. America: & again
could suppose that I had not thought of volume
touched on Geograph. distrib. He ignores also everything
that I have said on Obliteration, Retrograde
Succession. <u>Homologies</u>, Embryology & Rudimentary Organs.

500 He falsely applies what I said (this morning)
about "blindness of preconceived opinion" to those
who believe in creation; whereas I exclusively
apply to remarks to those who give the
multitude of species on two opinions, but believe
in remainder. —

p. 501. He slightly alters what I say, — I
<u>only</u> mention creationists really believe the
elevated claims have flashed into life.
He says I describe them as so believing;
& then insert in a "difference" —

501. He speaks of my "clamouring against"
all who believe in creation; & then seeks to me
an unjust accusation.

522 – He makes me say that the Dorsal vertebra vary – this is simply false: I believe I say a word about Dorsal vertebra.

525. What an illiberal sentence this is about "any pretension to condemn" & about my making those branches which stopped Lucian: such an argument would stop any progress in science.

530 How disingenuous to quote from my remark to you about my <u>brief</u> letter, as if it applied to whole subject.

530. How disingenuous to say that we are called on to accept the theory, from the imperfection of geological Record, when I over & over again have given a difficulty – the imperfection offers. –

519. How often the abominable assertion Bear has been made to worry me; but Bear is false, when he says the theory has no application, as put in my 2nd Edit, for it certainly works illustrates "diversified & changed habits" of life. –

512. It is a good jokes that the Reviewer is so full of Owen's works that he refers to "On the Nature of the Limbs", when he means to refer to my book & gives wrong page: but attends to quotation.

Le Goff

37

LONDON-S.F.
X
MY 24
60

5c

Prof. Asa Gray
Cambridge
Boston
Massachusetts
U. S. States

L
MY25
A

under 1/2 oz. –

number means to blow me into atoms. He does not in least appreciate the difference in my views & Lamarck's, on explaining adaptation, the principle of divergence, the union of dominant groups, & the almost necessary extinction of the less dominant & smaller groups &c. &c. —

Talking of adaptation, I have lately been looking at our common orchids, & I daresay the facts are as old & well-known as the hills, but I have been so struck with adaptation to the cantharides, that I have sent notice to Gardeners Chronicle. The Ophrys

Down Bromley Kent
June 8th

My dear Gray

I have to thank you for two notes, one through Hooker, & one with some letters to be posted, which was done. — I anticipated your request by making a few remarks on Owen's Review. Hooker is so wary of Reviews, that I do not think you will get any hint from him. — I have lately had many "more kicks than half-pence" in reviews in Nature. Sedgwick hates that. Mag. is the most unfair thing which has appeared, — one mass of misrepresentation. It is evidently by Haughton, the geologist, chemist & mathematician. It shows immeasurable conceit & contempt of all, who are not mathematicians.

He ~~in~~ discusses Bee-cells, & puts a series, which I have never alluded to, & ignores the intermediate cells of Melipona which alone led me to my notions. The article is a mixture of unfairness & arrogance. But as he meets at Malthus, I am content, ~~that~~ fact is clearly he cannot reason. He is a friend of Harvey, with whom I have had some correspondence. Your article has clearly, as he admits, influenced him: he admits to certain extent natural selection, yet I am sure does not understand me. It is strange that very few do, & I am become quite convinced that I must be an extremely bad explainer. To recur for a moment to Owen. He gives Huxley misrepresent & is very unfair to ~~Owen~~. You say that you think the article must be by pupil of Owen; but no one fact tells so strongly against Owen, considering his former position at Coll. of Surgeons, as this he has never treated one pupil as a follower. — In the number just out of Fraser's magazine, there is article or Review on Lamarck & one of W. Hopkins, the mathematician; who like Hallpeter despises the reasoning powers of all naturalists. Personally he is extremely kind towards me; but he evidently is following

aspiration, often, as you will see,
a curious contradiction in
illustration. — I get on very
very slowly with my larger work;
& of late from a very unhappy
cause, my poor eldest daughter
about 16 years old, has been
40 days ill with her fever:
we have had a good deal of
anxiety, but I hope & think
she has at last turned the
corner, though the fever has
not gone. — Farewell. When
you wished, pray write; — it
is a great pleasure to me to
hear from you. — Your most
truly & gratefully C. Darwin

over

P.S. I have been making some little trifling observations which have interested & perplexed me much. I find with Primroses & Cowslips, that about an equal number of plants are thus characterised

So called (& real) female plants

Pistil much shorter than stamens; stigma rather smaller; pollen grains large, throat of corolla short. –

So-called male plants

Pistil much longer than stamens, stigma simpler – pollen-grains smaller, – throat of corolla long.

I have marked a lot of plants, & started to find the so-called male plants barren; but judging from the feel of the capsules, this is not the case, & I am very much inclined at difference in the size of pollen, which I measured many times & always found 2 to 3 times as much left in the "male" than in the "female" plants. If it should prove that the so-called male plants produce less seed than the so-called females, what a beautiful case of gradation from hermaphrodite to unisexual condition it will be! If they produce about equal number of seed, how perplexing it will be. –

with a vengeance; & it will be a
blessing to me if all my friends
do not get to hate me. But
I look at it as certain, if I had
not stirred up the mind 10 or 20
years sooner; so that
to 100 years the bottle is fought the
longer it will be settled, — not
that the subject will be settled
in our lives' time. It will
be an immense gain, if the
question becomes a fairly open
one; so that each man may
try his new facts on it pro
& contra. — Very many
thanks about N. G. Times; I
daresay you will be disappointed
with the article; & I cannot
for the life of me tell what
it is that struck Gell & me

Down Bromley Kent
July 3d

My dear Gray

I received the mathematical papers,
for which many thanks, but have
not yet had time to understand
them: your letter of June 17th has
been forwarded to me here (for I
am at Water-Cure). I have had
an unhappy month of late: my
eldest girl has had low fever, 9 weeks
in bed, but at last we have moved
her: & my health quite broke
down. We shall soon have to take her
to sea-side & I want a considerable
change; for from anxiety & consequent
ill-health have done hardly anything
for last six weeks. — I am very sorry
to hear how extremely hard you are
worked with work. It is a pity the

you should spend more time over Reviews of my Book: you have done an immensity & been of incalculable service. I feel very grateful; though I know with that this is not a personal affair: you wish that the subject shd. be fairly treated & discussed. Nevertheless I cannot help feeling deeply obliged to you. — I can not very plainly see from many late Reviews, that I shd. have been fairly annihilated, had it not been for 4 or 5 men, mis==claiming yourself. The early favorable reviews were highly important in preventing the subject from being "boycotted." — Yesterday I had letter from Hooker at B.

efore.[n] at Oxford; & he tells me that there was one day, on my Book a savage fight between Owen & Huxley; & subsequently a discussion of almost warmth of 4 hours duration (!!) on a paper I drew of U. States on the subject, in which somehow (I know not how) my book became subject: Bishop of Oxford, one of most eloquent men in England, ridiculed me at great length & with much spirit; & Hooker answered him, I imagine, with wonderful spirit & success. — Owen with not pure sight, when he said that the whole subject would be forgotten in 10 years. My book has stirred up the mud

with respect to fertilization of insects.
The insect led me to find the
two horns (I subsequently dissected out
pollen-tube) in Gymnadenia conopsea
on the Stipers & you never saw
anything so pretty as the contrivances
are. Do you know Hooker's paper
in Phil. Transact. on Listera;
he misapprehended meaning of his
facts. I find the arrangement so
delicate that the explosion takes
place by touch of human-hair,
& the fluid sets hard, in under two
seconds. It was really beautiful
to see a little Syphus licking the
labellum, & as soon as it had
touched the arrangement the explosion
took-place, & the insect crawled
off with the 2 pollen-masses
cemented to its forehead, ready to





visit it — I hope I may find it at
home when I return there in
3 or 4 days. — I will order the
two numbers of the Atlantic when
I know which matter contains
your article; as I shall be
very anxious to see them.
I have just received your letter:
in truth I am myself quite
conscious that my mind is
in simple muddle about
"designed laws" & "undesigned
consequences". — Does not
Kant say that there are
several subjects on which
directly opposite conclusions
can be proved true ?!
I forgot to say that in Fraser's
Magazine for July, there is ~~Hung~~
Hung

article of our great Man W. Hopkins.
It is written with my fair spirit
& with not more of the arrogance
of a mathematician, than might
have been expected. I have remonstrated
with him for so costly raging
that I based my views on what
I took as great difficulties. —
Anyone of taking these difficulties
alone can make a most strong
case against me. I could
myself write a more damning
Review than has a sort
appeared! On question of hybridity
he supposes over a miserably fine gradation
from utter sterility to complete fertility —
— the fertility (W. Herbert) of some hybrids,
& the sterility of the cross of Verbascum
(Gärtner) & of the cross of Tobacco
(Kölreuter), which latter facts you
, & to my, never notice. — Your letters are
great pleasure to me, but pray do not
write whilst so overworked. —
I have this minute had letter from Bell;
who is just starting for continent;
on his return he is going to
investigate reported cases of Hippopotamus
subsequent to glacial epoch: he
finds Falconer & Prestwich are believers
in the remarkable fact; of so
then has, probably been in Europe a period
subsequent to glacial warmth. Do
you remember my saying that I
hoped I shd be proved wrong to
punish me for disbelieving in you;
& it seems that my punishment
is at hand! — Not being able
lately to work I have amused myself
about Orchids. I have been struck
with amazement at beauty of contrivances

unpaged next flower, with which it mated ~~it~~.

One word more on "designed laws" & "undesigned results". I see a bird which I want for food, take my gun & kill it. I do this designedly. — An innocent & good man stands under tree & is killed by flash of lightning. Do you believe (& I really ought to like to know) that God designedly killed this man? Many or most persons do believe this; I can't & don't. — If you believe so, do you believe that when a swallow snaps up a gnat that God designed that that particular swallow should snap up the particular gnat at the particular instant?

I believe that to man & to great ape in same predicament. — If the death of neither man or great ape designed, I see no good reason to believe that their first birth or production is to be necessarily designed. Yet as I said before, I cannot persuade myself that electricity acts, that to trees grows, that man aspires to lofty conceptions all from blind, brute force.

Yours muddled & affectionate friend

Ch. Darwin

10

Down Bromley Kent July 22d

My dear Gray

Owing to absence from home at Water-cure & then being to nurse my sick girl to whence I am now writing, I have only lately read the discussion in Proc. American Acad. And I now I cannot resist expressing my sincere admiration of your most clear powers of reasoning. As Horkan lately said in a note to me, you are more than a match for the thorough master of the subject. I declare that you know my Book as well as I do myself; & bring to the question new lines of illustration & argument, in a manner which excites my astonishment & almost my envy! I admire these discussions, I think, almost more than your articles in Silliman's Journal. Every single word seems weighed carefully, & tells like a 32-pound shot. It rejoices me much (but I know that you have not time) that you could write more in detail; & give, for instance, the facts on the variability of the American wild fruits. The Athenaeum has largest circulation & I have sent my copy to Editor with request that he would republish the first discussion: I much fear he will not, as he seemed subject is so hostile a spirit & is not a liberal man. — I fear that I shall lose my copy. — I have received p. 416 to 418 — and p.p. 425 to 434 —, so that pages 417 to 424 are missing.

I wish you would take in Nature I could buy these sheets of Proceedings. — Your Gorilla Tiger had not arrived when I left home. — Hooker has lent me (& we must struggle about authorship!) the Jul. Monthly Atlantic: it is uncommonly pleasantly written, & will take well in politics. — I shall be curious (& will write) to August number, & as soon as I know that it contains your Review of Reviews. — My conclusion is that you have made a mistake in being a Botanist, you ought to have been a Badger, & you would have rolled in wealth & penetrating the truth, instead of judging the truths of this world. — Of Reviews I have to report one in Quarterly by the Bishop of Oxford (Wilberforce) evidently written: it is uncommonly clever, but with anything scientifically, but gingerly in splendid style the I chuckled with laughter at myself — for the that I gather, M. B. after at Oxford addressed subject greatly, owing to effort of Hooker, Huxley & Lubbock, notwithstanding important attack. Hooker & Darwin are shaken. — I hear from Hooker that he hears from Hooker letter the my views are making my considerable progress in Germany; & the good writers are hinting to question. Brown his translation has a chapter of Criticism; but it is such difficult German, that I have not yet read it. Hopkins Review in France is thought the best shot has offered against us; I believe the Hopkins is so much opposed because his conception of Deity has been led him to reflect much on such subject as Geographical Distribution, Classification, Homologies &c &c; so that he does not feel it a relief to have some kind of explanation. — I see most clearly that my Book would have been a dead failure, had it not been for the persevering labour bestowed on it (not for my idea, but for subject idea) & generally, Hooker, Huxley & Carpenter; & to these names I hope some Geologists may be added. — But I know the Quarterly will cause Geologists to shake in his shoes. Considering his age & position in Society, I think his conduct has been heroic on this subject. — I am tired, so will write no more. Remember my kind & good friends.

Yours most truly

C. Darwin

If you see Prof. Parsons, will you thank him for the exceedingly liberal & fair spirit in which his Essay is written. — Please tell him that I expected much as chance of formidable opposition [i.e. great & sudden variations] arising. I have, of course, no objection to them; indeed it is a great aid; but I did not allude to subject, for after much labour I could find nothing which satisfied me of the probability of such occurrences. Then seems to me in almost every case too much, too complex, & too beautiful adaptation in every structure to believe in its sudden production. I have alluded under head of beautifully hooked

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Down Bromley Kent
Aug. 11th

My dear Gray

On my return home from Sussex about a week ago I found several articles sent to you. — The 1st article from the Athenæum which I am very glad to possess. By the way the Editor of Athenæum has inserted your answer to Agassiz, Bowen & Co. & when I therein read them, I admired them even more than at first. They really seem to me admirable in their condensation, force, clearness & novelty.

I am surprised that Agassiz did not succeed in writing something better. How absurd that logical quibble; — "if species do not exist

how can they vary?" As if anyone doubted these temporary existences. How coolly he assumes that there is some clear defined ~~difference~~ distinction between individual differences & varieties. It is no wonder that a man who calls identical forms when found in two countries distinct species, cannot find variation in nature. Again how unreasonable to suppose that domestic varieties selected by man for his own fancy (p. 147) ought resemble natural varieties or species. The whole article seems to me poor: it seems to me hardly worth a detailed answer (even if I could do it, & I much doubt whether I perhaps you still in picking out salient points & driving a nail into them) & indeed you have already answered several points. Agassiz' name, no doubt, is a heavy weight against us; but yesterday I heard that a man, whom I believe to be greater than Agassiz, viz Von Baer goes a long way (how far I know not) with me, & has spoken out publicly & with probably publish. R. Wagner has published, also, in Germany an abstract of Agassiz' Essay on classification, & say he believes the truth lies between us two; & this will make A. very savage, I ph. think.

seeds to such fertility. Monsters are apt to be sterile, or not to transmit monstrous peculiarities. Look at fineness of gradation in the shells of successive sub-stages of same geol formation. I could give many other considerations, which make me doubt such view. — It holds to certain extent with domestic production no doubt, where man presents some abrupt change in structure. It amused me to see Sir R. quoted as a guide of affinities of animals; & it gave me a "cold shudder" to hear of anyone speculating about a true Centaur giving birth to a true Fish!

Since I wrote last I received your note of July 10th, in which you confirm to capital big story. (I fear Prof. Wyman will never give us his facts about stuffed horses)

Also you give us valuable hints about diœcio-dimorphic flowers. — I was forced to gather my cowslip seed too soon, & I am all at sea about the difference in fertility; but I shall try & work at it a little better next summer. —

My poor daughter is decidedly better, though this very cold & wet. But I hope the organic mischief suspected by the Doctors, consequent on the fever, is slowly righting. There was fluid in the abdomen, but this seems to have absorbed; but there is still some hardness. We have had a miserable time of it. —

Farewell my kind friend

Your most obl.

C. Darwin

two copies of October number; to do
not trouble yourself to send it.
By the way I am thinking of
taking a very great liberty; but
after much consideration I do
not think you can object:
you said that it was known
that you were the author of
the 1st article; & as the
best chance of getting it reputed
in England in a scientific
journal will be to affix
your name. I think of doing
this & I hope to receive the
proofs with not thinking this
an unwarrantable liberty.
I think most highly of this



Down Bromley Kent
Sept 10th

My dear Gray

On receipt (through Hooker) of your
letter of Aug. 26th, I ordered my
Bookseller to send you Hopkins'
2d article in Fraser; it was very
stupid in me not to have thought of
sending it & to Quarterly. But I
presume the Quarterly will have
long ago arrived in States.
I have ordered money to send
the "Origin" to the American
Acad. of Sciences, to your care,
as I did not know proper address;
I shall be grateful for returns.
Many thanks for to sheet replacing
those sent to Athenaeum. I
sent the copy of this Athenaeum

of Bell's request to him in Germany
(when he is fossil-bone hunting)
& he writes to me that he
thinks the argument of your
quite admirable. — I have
not yet received to papers
mentioned in your letter sent
to Hooker, for H. first lent
them to Lady H. — You will
have heard of Hooker's
astonishing expedition to Syria:
if he ascends Lebanon,
it — my answer scientifically,
but I cannot help thinking
the expedition rather rash. His
absence is so great a loss to
me, that I am hardly fair
judge of the wisdom of the journey.
I have got, nevertheless, your second
article in Atlantic monthly; for
a copy of which number was sent to my
Brother-in-law Hensleigh Wedgwood,
on account of a Review of
his Dictionary. Do you know
who the writer is, as my Brother
would like to know??
I have ordered another copy of
this August number, as I shall
like (if I can get it in Sydney)
to send copy to Pictet; &
I have been thinking of whether I
consult Huxley, whether I could
not get it reprinted in
some English Journal. — I
have, also, advised of anticipation

was no design in to death
of each individual Fly. —
Farewell my good Friend

Yours most truly
Charles Darwin

Pray write to me
when too much pressed
by labour, — though I
value much your
letters. —

Article & I cannot bear to
think it shd not be known
in England. You will be
very of my praise; but it
does strike me as quite
admirably argued; & so well
& pleasantly written. — Your
many metaphors are invariably
good. I said in a former
letter that you were a Lawyer;
but I made a gross mistake,
I am sure that you are a
poet. No & future I will
tell you what you are,
a hybrid, a complex cross of
Lawyer, Poet, Naturalist, &

Metapiscis! — Was there ever such a monster seen before? I have just looked through the papers which I have marked as appearing to me the good, but I see that they are too numerous to verify; & there is no exaggeration. My eye just alights on the happy comparison of colours of prism & the artificial group. — I see on little extinct of fossil cattle in S. America. — It is curious how each one, I suppose weighs argument in a different balance: enlargedness is to me far strongest weight class of facts on favour of change of forms, & not due, I think, if my reviewers have alluded to this. Variation not coming on at a very early age, & being inherited at not very early corresponding period, explains, as it seems to me, the grandeur of the fact in Natural History, a rather, in fact, in the resemblance of embryos. —

Hensleigh Wedgwood, above alluded to, is a very strong theist, & I put it to him, whether he thought that each time a fly was snapped up by a swallow, its death was designed; & he admitted he did not believe so, only that God ordained general laws & left the result to what may be so far called chance, the then

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Down Bromley Kent Sept. 28th

My dear Gray

I have a multitude of things to thank you for; & hardly know how to begin. I forward a stamped letter to Gilchrist in your of Sept. 2d. Always use one this way, when convenient; as a check for you, I consider my good payment. With respect to Dr Baer's statement about Guinea-pigs; I rd. much like to know where it is; but it can hardly be trusted unless he has brought forwards some quite new evidence with respect to the unknown wild type of Guinea-pig: I have seen some of the Hybrids of Rabbit & Hare; & believe can vis to be trusted; but I do not know that to your half-words are perfectly fertile inter se: it is a particular curious case, because many have persevering tried & utterly failed even to unite them 2 species.

Your letter of the 2d was very pleasant, & so was that to Hooker, which (pink covered for Lady Hooker) I rd. well & all (i.e. my family) laugh heartily. I have received the Dictionary from Silliman' Oct. no. Also the Proofs of the 3d. Atlantic Article. These both seem to me very good; but if forced to choose I rd. pick out the 2d article as (I don't know though I think) I do not pretend to be a good judge, of my best. — I have never attended to Logic, Philosophy &c; but it is my opinion that you are best reasoner, of any man, let him who he may, that I ever read.

as for your metaphors & similes, they neither me entirely: I wd like to steal a few: yet you never are tempted to use them prodigally. The two last essays are for the best Theistic essays I ever read. But I must return to the metaphors — I like especially that of the woman & the cloth; and that of the rain-drops on the ocean. — I believe my chief difference with you, is that I (unfortunately) think more of the rain-drops on the ocean than on the land. All your arguments about design seem to me excellent; but I must handle them all again. I have a feeling that the existence of the multitudes of stars & the motion of the planetary system are equally good with living beings, to prove a first cause & get rid of those who in living things, there are hardly any design. — But I will know this subject. — I have sent the manuscript-bundle on to a mag. of h. thirty; & plead to argument in Atlantic for its insertion: pay for its printing, if its length prevented its insertion: I have affixed your name, as Sybil agreed that it wd gratify added to its value: I do not imagine that I have not done wrong. The Saturday Review has lately been discussing Design; so I will send the Dialogue to it, of course without your name; whether they will print, I doubt. — I will I not think of any journal to print the ~~Dialogue~~ October number. — I have had a letter of 14 folio pages from Harry against Book, with some ingenious

May 28

& new remarks; but it is an extraordinary fact that he does not understand at all what I mean by Nat. Selection. I have begged him to read the Discussion in Nat. Hist. Review, as you never touch the subject without making it clearer. I look on it as a most extraordinary fact that you never use an word or epithet which does not strictly express meaning. Now Gell, Huxley & others, who fully understand my book, get sometimes quite perfectly to think I demur. If I had to write a popular book again I would call it "Natural Preservation" & "Selection", but it is too late now. Dr. Gray says to me "it is, you know, strictly true that there can be any Selection in Nature" I made no answer; for it is hopeless. Well, your extraordinary labour is over; if there is any fair amount of truth in my views, I am well pleased that great labour has not been thrown away. I am perfectly convinced that had it not been for yourself, Huxley, Hooker & Gell; my Book would scientifically have been a complete failure. You have been most generous & disinterested in your exertions; & I thank you from my heart.

Farewell; my dear Gray

Yours most truly

C. Darwin

I get hope, & almost believe, that the time will come when you will go further in believing a very large amount of modification of species, than you did at first & do ~~now~~ now.

Can you tell me whether you believe further a much strong than you did at first? I shd really like to know this. I can recollect in my memory correspondence with Gell, who objected to much at first, that he has, perhaps unconsciously to himself converted himself very much ~~since~~ during last six months. — I think this is case even with Hooker. — This fact gives me far more confidence than any other fact. —

P.S. 1st

How does Dana get on? I wrote to him, a month or two ago. —

P.S. Please observe that if the application loss of the 2d Edition next calling, I shd. prefer keeping the money they have paid me. —

I am writing this at Eastbourne, where we have come to recruit for girls health, & shall be certainly delighted at. — Consequently I shall not see Clark (Aspasia man).

I have been sedulously amused of working at Drosera: the movements are really curious; & the manner in which the leaves detect certain nitrogenous compounds is marvellous. — You will laugh; but it is at present, my firm belief (after a fresh experiment) that they detect (& make use of in consequence) the $\frac{1}{2880}$ part of a single grain of nitrate of Ammonia; but to manipulate a sulphate of Ammonia, both their chemical skill, & they cannot make anything of the nitrogen in these salts!

I began this work on Drosera in relation to gradation & throwing light on Dionaea. —

Does Spiranthes (or any odd genus of Orchids) grow near you; if so, I would ask you next time to make a few observations. It would not take 10 minutes to me. There is my curious conference in one of autumnal for insect fertilization; viz a decked boat-full of stinking matter & on a truck the dead effluvia & lovely insect matter spread. But I wish you have a Spiranthes. —

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Down Bromley Kent

Oct. 19th

My dear Gray

Your pleasant letter of Oct. 1st has been forwarded to me to sea-side, & I have been meditating on it. – First let me say how pleased I am to hear that Prof. Henry goes to Jan with me. – When I sent to Ag. Atlantic & Atlantic, I said that I myself took the responsibility of adding your name as author; so that I do not think it will be made to appear as if from you. – Nor do I at all know whether it will be thrown printed; but the Editor is not my correspondent & I do not like writing to him again. – I enclosed envelope, begging for it to be returned if not used; & I have not received it. But this does not convince me that it will appear; but it is either now rejected or printed, so much time has elapsed, to no time for 2 or 3 months. But I have been thinking of a

larger scheme, viz to get to whole thing published with (as you permit) your name.
But unfortunately I am the worst person in
world, from mixing so little with people,
to publish; & Hooker is away & busy
in Sikkim. But I intend writing to
Dr Lyell, who admires your services
as much as I do (especially I think his 3/
& has written me several letters, chiefly about
them). Now he is a real good judge
in all publishing affairs & if he thinks
there is any chance of even a small
sale, I will consult Murray. I
think I may say to Murray that a Dr. Burn
has just published a Book against
the Origin & two other Brochures
have appeared (all bosh), a pamphlet
on our side (to a certain extent) might
sell. It would not be worth while to lose
money, not for the money sake, but on account
of small circulation & consequently not much
good being done. It would be indispensable
to have your name & title on Title-page;
& my advice is to have some remark
on Title, showing its being a Selected
Thesis & Design. — I daresay it is
10 to 1 against my succeeding; but, as it
will cost my little trouble, send me a
Title, (subject to Murray's approval). I
fear there is no chance of your having
time to add some criticism or
Hopkins. — I will let you know as
soon as I can see my way. I must
wait till I get home & see how
long all these Articles are together.
They seem to me much too good to be
almost lost in a Periodical —

Murray says the Origin goes on selling well. —

I had a letter a week ago from Thwaites of Ceylon: he was at first much opposed to me. he now says "I find that the more familiar one becomes to your views in connexion with the various phenomena of nature, the more they commend themselves to my mind." —

Even Harvey, I find, is not nearly so savage against me as he was when he published his foolish pamphlet. Such cases give me much confidence that Natural Selection is not very far from truth. —

Believe me
My dear Gray
Yours
C. Darwin

Perhaps it would be worth while for you to write & send me Title, & say whether you would add any criticism; or in this case I could delay publication, if it is near to take place. —

Oct. 24 15 Marine Parade

Eastbourne

1860

My dear Gray

We are in much difficulty as my poor girl has just had relapse, but is rallying again. — To save you labour in vain, I write hurriedly to say that Huxley, like a good & kind man, has been consulting with Murray about publishing your Review as pamphlet; but they say, (though it is against their rule to publish pamphlets, they would break it through this time) that

put in 2d Part; & if they do I will try it as for 3d Part; & I will offer to bear the expense of petty & printing if they will condescend to accept it. I cannot bear that such admirable Essay should not be presented in this country. — I have no idea whether Sat. Rev. will insert Review. —

God Bless you

C. Darwin

they do not advise it. That it would be absolutely necessary to spend more in advertisement than the cost of publication; but that then would not suffice, that in short, they are well convinced that it is impossible to get a pamphlet circulated. They say they will do it "but it will be a dead loss". Under these circumstances it would be no use to attempt it. I am much vexed. —

Lyell personally concurs with Murray's view. —

Lyell has highest opinion of the "Labour & Wages" than on other 3 Reviews — In short agreeing quite with me & he cannot wish go further. He adds "it would be well worth "while if a little Book "could be got up of this "very fine Metropolitan "part in 10 admirable L "would surely have many "readers". —

I now hope accounts of Mr. Hunt will

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Oct 31 1860

15 Marine Parade
Eastbourne

My dear Gray

Since I wrote about a week or 10 days ago, we have had such a week of misery as I did not know men could suffer. My daughter grew worse & worse, with intolerable suffering, so that all the Doctors thought we should lose her. But the stoppage is over, & she has rallied surprisingly; but whether there is much organic mischief & what the final result will be cannot be known, the immediate issue is decided. But she is quite easy now, & she comes at last to sleep easily for the first time. & we have managed to conceal from her, her extreme danger. — You are so kind & sympathetic that I have not resisted telling you my unhappiness. — We shall not be able to remove her home for several weeks, even if the case is not worse than the Doctors now hope & believe. —

I received this morning your letter of Oct. & forwarded that to Decaisne. — Thanks for Atlantic Oct. No.^2 which I shall find at home; when I have obtained 2 copies for myself, so I shall be

me to give away. How I wish you had time to write an article in relation to descent with modification: how well you would do it — and you have some time bring it in a side wind in discussing affinities of some group in some of your papers! You have in truth far more, over & over again, given me than you imagine in getting my views a fair hearing. — I have been reflecting about getting, as you suggest, if it can be done, ~~[illegible]~~ 250 or 200 copies of the 3 articles of the Atlantic reprinted from to Globe in America & sent here. Could it be done for 4 or 5 £ and I would gladly pay that. — I do not think it would be my material about the raising. But what I do consider all important is that you would put a title-page with your name & titles. If you did not object to this, I would post a copy to all to Scientific men I could get addresses of from the Societies. Which your name was appended, the articles would not, I am sure, be read in England & it would be time & trouble thrown away. Their merit would not be discussed. But would it not cost you a good deal of trouble to get it done? My book might be sent to care of Murray Albemarle St Williams & Norgate, marked "to be forwarded". — ~~[illegible]~~

I am profoundly gratified with your sentence "I think it grows more & more likely that species are defined." I am grieved to hear about Dana. — I enclose sketch about Spitzenbergh; if you will observe your species, I fear be infinitely sliped. If a very distinct species, the contrivances will probably differ; as the contrivances are entirely dissimilar. — I am not sure that you will understand my rough description; but you will if you species is like nuts. — I shall publish on Dorsen. —

Your very obliged
Most truly C. Darwin

as you speak of determinate movements for an end in plant I am tempted to give case of Orchis pyramidalis which seems to me pretty. The pollinia

are attached to connate glands of shape of a saddle, & are suspended in cup of liquid with a nectar lip. – Insert a needle into the nectary, & the lip of cup is moved, & the sticky under surface of saddle sticks to needle & the pollinia are removed. Immediately the saddle is exposed to ~~air~~ air (& not whilst under water) the flaps of the saddle curl in & embrace the needle. I had a moth sent me with 3 pollen-masses attached to proboscis & the sender said it was astonishing how the moth had boxed through the sticky glands of some orchis! The movement of the flaps is very conspicuous. At first


pollinia occupy this position (viewed laterally, & pollen-mass behind the other); & if you push needle into nectary of another flower, the pollen-masses do not touch stigmas, which are lateral & double, one on each side of orifice of nectary. But immediately after the flaps of saddle have seized the needle, another movement commences, & the pollinia move downward,


nearly parallel to needle & the two diverge from each other. So if if you push needle into nectary, the pollen-masses exactly hit the lateral stigmas & leave pollen-grains on them. The seat of this latter movement, which is always in the direction, lies at the junction of the footstalk of pollinia & the saddle.

## Spiranthes autumnalis

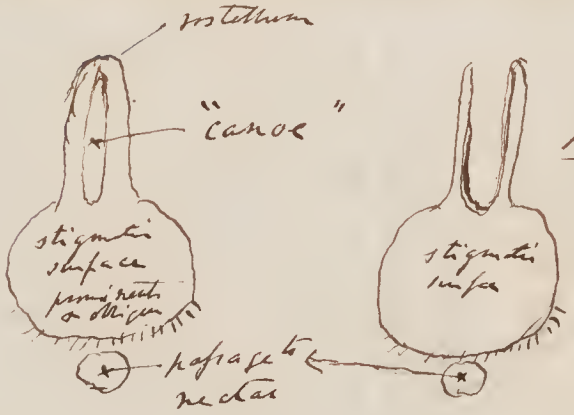



fig 2.

(1) In Kathy early bud the anthers open & press pollinia against back of rostellum

(2) Back of rostellum opens in early bud & a viscid substance glues the pollinia to the back of rostellum & sticks to the bottom of what I call the "canoe".

(3) When the flower first opens the rostellum is not ruptured on the front surface, (i.e. on the surface above the stigma)

indeed represented by fig 2. All to flowers which have been visited by insects are in this condition. Change the needle into the proboscis of insect, & you will see how fertilisation is effected. The pollinia become attached nearly parallel to proboscis; & the insect visiting a second flower is sure to strike the apices of the pollinia against the slightly projecting viscid stigma. If you poke thin culm of a grass down to nectar & withdraw it without bending, you will not withdraw the pollinia; but if before you withdraw it, you bend the culm so that its broad or convex surface touches the rostellum the pollinia will generally be withdrawn. Moths in curling up their proboscis after sucking, & perform this action; & I go so far as to believe, that structure of flower is made in relation to this mode of withdrawing proboscis!!!

Within the substance of the rostellum a hard brownish, which can be seen, which is of the shape of a "canoe", pointed at both ends, & standing vertically up, parallel to the longer axis of rostellum. The bottom of canoe, as stated, is already glued to the pollinia. — The front & concave or hollow side of the canoe is filled or loaded with very viscid substance, which sets hard in ~~about~~ less than a minute when exposed to the air. The concave side of the canoe is, as stated, covered, when flower opens, ~~with~~ by the membrane of the rostellum. —

(4) Now if front surface of rostellum be touched longitudinally & most delicately by a needle, (or exposed to vapour of chloroform) its mistakenly spirits longitudinally its whole length, & the needle comes into contact with the viscid matter within the canoe, ~~is found~~ The canoe embedded in the rostellum becomes firmly glued to the needle; & the bottom of the canoe is already long since glued to the pollinia. Hence when the needle is withdrawn the canoe & pollinia are all withdrawn together. The rostellum is then left in the state

27

Nov. 26 — Down Bowly Kent.

My dear Gray.

I have to thank you for two letters: the latter with corrections, written before you received my letter asking for an American Reprint & saying that it was hopeless to print your Review as a pamphlet, owing to unfavorableness of getting pamphlets through. — I am very glad to say that the August or second Atlantic article has been reprinted in America & big of the History; but I have not yet seen it there. Yesterday I read over with care the third article: & it seem to me, as before, admirable. But I grieve to say that I cannot honestly go as far as you do about design. I am conscious that I am in an utterly hopeless muddle. I cannot think that the world, as we see it, is the result of chance; & yet I cannot look at each separate thing as the result of design. — To take a typical example, you lead me to infer (p. 414) that you believe "that variation has been led along certain beneficial lines": — I cannot believe this; & I think you would have to believe, that the tail of the Fan-tail was led to vary in the number & direction of its feathers in order to gratify the caprice of a few men. Yet if the fan-tail had been a wild bird & had used its abnormal tail for some special end, as to sail before the wind, unlike other birds, everyone would have said what beautiful & designed adaptation. Again I say I am, & shall ever remain, in a hopeless muddle. — Thank you much for Bowen's 4th Review. The contempt with which he treats all animals to be destitute of reason is simply absurd. It is monstrous at p. 103, that he should argue against the

possibility of accumulative variation & actually leaves out entirely Selection!
~~He could be~~ The chance that an unpaired Short-Horn, or unpaired
Porter-pigeon should be produced by accumulative variation, without
man's selection is as almost infinitely to nothing; so with
natural species without natural selection. How capitally
in the Atlantic, you show that Geology & history are
according to Bacon Metaphysics; but he leaves out the
subtle in the 4.to memoir. — I have not much to
tell you about my Book. — I have just heard that
Dubois Reymond agrees with me. The sale of my Book
goes on well, & the multitude of Reviews has not stopped
the sale. Murray sold a few days ago at his sale
300 copies; & he has not half; so I must begin at
once on a new & corrected edition. — Hooker has returned "very
fat & jolly". I am glad to say; but I have not seen him
& not heard much news; except that he found traces
of glacial action on Lebanon. — I have gone on working
at Drosera, but shall not publish this next summer, as I am frightened
at my results & must retest them (By the way I have been
reading in consequence some part of your Lecture in Botany, & have
been so much pleased with the striking clear way you
put things), but you may rely on the truth of the fact
that the prolonged weight of an insectivorous alga,
placed with all care on one of the glands, caused conspicuous movement.
it weighed only $\frac{1}{78,000}$th of one grain
I got the weight by weighing a length of fine hair & cutting
off atoms & measuring them with micrometer.

I will send you a copy; for chance the sun
may possibly shine; but good Heavens
how sick you must be of it. —

This weight is 78 times less than that with which best balanced with
those with; & yet you may rely on it, this
suffices to start the movement. Moreover it produces
such changes within the cells of the glandular hairs;
that an hour after weight had been put on,
I could distinguish which hairs had carried this
fairly weight from the other 100 or more hairs on the
leaf. — I suppose in summer you take walks
in the country: I see in your Flora, you say that
Ophrys apifera & aranifera in common & another species,
will you learn whether the flowers of both species catch number of flies
by their proboscis, as the former does in England. Next
Bees visit the flowers. I mean to get this
whether
plant, if I can, & learn it; as a Boy I was inspired
at number of flies captured. Then make a memorandum
about this plant & the Spiranthes. —
My daughter improves very slowly & I have not the
heart to work again very on hard (strictly
on light) as I ever can. — My dear Gray
Ever yours most truly
C. Darwin

Have you read Olmsted's Journey in the Back Country;
what a remarkably interesting Book. —

article was what he
wanted. — I know nothing
about payment, but will
enquire & let you
hear. — In haste
Ever yours
C. Darwin

I heartily accept
either in better or
cheaper edition —

Hooker will I am sure
help in getting pamphlet
known. —

38

Dec 11th Down Bromley Kent

My dear Gray

I have just got your letter of
Nov. 26th. — Not to lose the
first chance, I write to
say that I will gladly
pay half of the (about)
20 £, & will distribute
copies & leave others in
the agents or Trübners
hands: I will put a few
advertisements in newspapers & see
what I can do to get
it noticed in a few
Periodicals. — But I am

a bad man for that, as I like to retired. —

I must hear somehow to whom the consignment of the copies are sent & you can tell me exactly how to repay you. — I am heartily glad of this. But I fear it will be impossible to get it known in England. So as to sell many. —

If upon reflection you would not like to go to Yarrow do not consider, of course, yourself bound. —

Just to verify use of your pamphlet. Bishop of London was asking Huxley what he thought of the Review in Quarterly, & Huxley's answer was that "the Gray in it Atlantic!"

I will write to Huxley about Mr. Wright's article but I have no hope, as H. said a good strong anti-Darwinian

P.S. I have just received
your note & the enclosure from
Asa Gray — How can you
imagine there can be a
doubt about the reply? Of
course I shall be exceedingly
glad to have it if it is
good — I wish to see articles
both pro & against. In any
case it is clearly understood
that the Review is a 'Champ
libre' —

Paying for it however
is what I unfortunately
do not see my way to
at present — Yours

the circulation reaches
or 7 hundred we should
hardly be able to pay anything
& even then I am afraid
not much — But get
the article if you can &
if your friend is the most
rabid advocate you can had
so long as he argues the question
scientifically & isn't abusive
(beyond reason) I am quite
sure Brooks will agree with me
in the propriety of inserting it
My desire to have an anti-
Darwinian article — is not
so much because I would love
you criticised & because I would
like the world to understand that
it is not a party journal. Will he
& return G. J. Holyoake's proofs for us?

39

Dec 14th Down Bromley Kent

My dear Gray. The enclosed note of Huxley's you will see shows that he will be glad of Mr Wright's article, if approved of; but he cannot pay for it. I really do not believe that there is any other Review which would accept it, for all have treated of the Origin. — If Mr Wright cannot better dispose of it; it had better be sent to Mr Williams & Norgate Henrietta St, Covent Garden; as they publish the Review — I feel sure that the Review will have great merit, & is far be worthy of Mr Wright's article; & it would be a great pity if it were not published, as you say it is so good. — You will see that Huxley asks whether you would ever contribute to his Review; but you must, I fear, be pledged to Gillman's.

I shall be anxious to hear your final decision as to manner of publishing your Theo Article. I

others think No. 2 best, though I believe most would prefer No. 3.

I shd rather like to see Mr Rose's Harvey's letter. — I have had much correspondence with him; & he wrote me a very nice letter.

I return Tickner & Fields letter, as you might want to refer to it. — My dear Gray

Yours most truly
C. Darwin

There is a nice, but too flattering article on the Origin by a young man, whom many think most highly of. —

12

23. Dum

My dear Gay

Your very kind letter, as my poor girl has just begun severe attack. — I was very glad to hear that you have decided to publish. Please remember a great on person the I bear to who made risk of life I am a rich man. —

You will not misunderstand me. —

The enclosed explains itself.

Price of 2s — strikes me as too high. —

I copy with I made few days ago of first lay, which I would be myself distribute. — I have just

received letter from Prof. Wyman. – I have not read it, but I see it is perfectly interesting to me. – Thank him. Yours most sincerely

C. Darwin

For Reviews

Annals of Nat. Hist.

Edin. New Phil. Jn.

Ath. Medium

Nat. Review.

Gardeners Ch.

Nat. Hist. Rev.

Zoological.

Geology. Soc.

Royal Soc.

Linn. Soc

Athenaeum Club

Hermione (a Review)

Private

J. C. Woodward

H. C. Watson.

Huxley

Bartram

Carpenter

Huxley

Gulliver

Self

Hemsher

Sedgwick

Hopkins

B. of Oxford

Herschel

Whewell

Lyell

C. Knight

Bunbury

R. Chambers

Wallaston

Sir H. Holland

Falconer

Hooker

Mr. Gould

L. Horner

Phillips

H. D. Rogers

Prestwich

Ad. de Candolle

Dr. Hooker

Thomson of Calcutta.

Blyth of Ceylon

Thwaites of Ceylon

I have just read your letter, which like all yours, interests me. You ask about Dissen: if you like to try anything, put the minutest atom (under a lens) on point of fine needle on any one single extreme marginal gland of a leaf, which has all the hairs equally expanded & watch it or look again in 10 minutes. — Or put fragment of hair of your head & look in a long time. I intend trying many more experiments this summer & then publishing: I am doubtful on many points. But the worst is that my health is failing much. I literally cannot listen to a word for ½ hour without fatigue. My good dear wife declares, I must go with me

54

Feb 19th — Down Bromley Kent

My dear Gray

I received your note of Feb 5th this morning. I am most sincerely sorry to hear of your accident to your thumb, which I cannot but fear will be a very serious loss. I will send your note soon to Hooker that he may hear about it. — I am glad to hear of the 250 copies and I presume the Jubilees: I wrote to him today about a few advertisements &c & to send me copies for distribution. I hope for my sake, as well as yours, that Murray & Sydenham will not pass entirely them, that it is impossible to circulate a pamphlet in England. —

I fear the United States must stop all interest in everything not political. — I will in few days enclose at my Bankers how I can repay you the £7. 10s: I trust with you would let me pay to whole £15. Will you not? I have asked Trübner to forward C. Wright's article to Porter quarterly. The Printers have been very slow with my new Edit. of Origin, so that I have been able to insert notice of your Pamphlet with title in full, which I am especially glad of. — The other day a very clever Lady was staying here & read your Reviews; & remarked "how shocking if such a writer has written out on Science" I need not say that she was not a scientific Lady. — With respect to Pumilio, you are quite right I know nothing whatever on subject, but I looked to answer myself, & asked Hooker, & he thought Smith would like an account for Q. Chronicle. It was foolish of Smith to put it as anonymous; in my note to him I said that he might like to use it sometime just to fill up space. — It strikes me, however, as pretty case of adaptation for the ignorant, like myself.) When I last wrote, after receiving your letter of Dec. 15th, I was in much distress about my pamphlet: the attack though very severe lasted only 4 days & I has now got up to state as before. —

trust hope that you thanks
will not cause you much
inconvenience.

Most cordially & gratefully Yours

Charles Darwin

If you want a book
for light reading, I think
of Leslie's Life (the American
Painter) is very nice.

whole family (if my girls can be
used) for two months to Water
Cure; & I fear I must, but it
will be grievous to all my
experiments. — I remember formerly
having read your extremely
curious speculation on Tendrils,
but I thank you for telling
me of them. —
With respect to designs you
say that you suppose that
I have "not brought forward
any such objections against
your views." — I have no
such objection, nor any
such foundation, nor any clear

view. – As I before said I flounder hopelessly in the mud. –

You have amused me much by your account of Geoffroy's denying the community of descent of allied languages, & of Bowen denying heredity. I cannot believe that Bowen is a strong man. What an odd & foolish fancy he must think it that the breeders of Race-Horses, Cattle & pigs do not keep pedigrees, & would certainly prefer breeding from a poor specimen of a good pedigree than from the finest of bad pedigree. – There were in fact work on my (I wish I dared say our) side.

Farewell. Do not forget to have look at Wallace's of your Siberian Flies. –

Farewell. – I must

57-7

Feb. 26 Down Bromley Kent

My dear Gray. My Bankers tell me that this was the only plan by which they could transmit to £ s. d. 7.10.0: I hope it will not be troublesome to you. Mr Trübner have been most liberal & kind, & say that they shall make no charge for all their trouble. I have settled about a few advertisements, & they will gratuitously insert one in their next periodical. If you have any opportunity, would it not be well to write & thank them? My say that 1s would be sufficient price, but as they nevertheless recommended 1.6, I have, of course agreed. I have distributed 100 copies to men of science & sent 10 to Reviewers, Libraries &c. I am heartily grieved at this. I believe my pamphlet will do real & material Selection, right good service. I think the Title very good, but I wish the Type had been better & larger, yet this would have cost more. I have not revised it yet; but I see long words with the hyphens. The share of the cost of Boy was only 12, & the advertisements will be a few pounds, for which I have told Trübner I am responsible; but I hope I shall with even this. Yet all this on that pamphlet will not sell. By the way the 7.10.0 just covers cost of my 100 copies at 1.6 each. If the pamphlet did sell, I wish inform you for chance of you being able to send over more. I wish heartily it would sell for my own sake & that 10 more profit might be sent to you. — Trübner informs

me that in Box there was a MS. bundle for me, (i.e. S. Wright's Review). I have written to Liverpool to enquire whether it could have been lost. When Box was opened at Custom House. — I hope it is not lost. — Perhaps you addressed it to Williams & Norgate or to Prof. Huxley. —

My dear Sir

Yours most sincerely

Feb. 27th. —

Chas. Darwin

I am heartily glad to hear about your thumb. —

I have had my stomach so upset of late, that I have resolved to go bodily & baggage for 6 weeks or two months, before May 1st, to Malvern for Hydropathy, which is the thing which seems to ~~decide~~ me. — Hearing this upset Dale note, & believe me your ever most truly & obliged

Charles Darwin

52

March 12th. Down Bromley Kent

My dear Gray

This note will be chiefly on business. — I received this morning Wright's article: I have hardly glanced at it, but rather fear it is too metaphysical-theological for me. Huxley will be here in few days, & I will hand it to him & he will decide. If it does not suit him; what on earth shall I do with it? I have received, Sec. Prof. of American Academy in 4 vols, — very many thanks for them. — I have received your note of Feb. 15th & am very obliged to you for allowing me to beat my uncle Josiah of publication. I am surprised & pleased at sale of new Edit. of Origin in America. — You note have received before this than

Bill for £7. 10. 0. — Your pamphlet will do the subject very great good, whether or no it sells. — I have had many letters about it; all full of praise — "truly admirable" says one, "I am lending my copy to one person after another". Another says, "has read nothing on the subject with anything like the satisfaction" — Another says he (i.e. you) "is a cunning fencer & believes in you entirely". — I wish this was half true; though I do not mean the cunningness, which is utterly mistaken & false. I sent copies to Wedd, Matheson & Maxwell; but had already sent copies to Hooker, Bentham, Harvey & Balfour; to the wife here despatches. I was much pleased, the other day, to note from Leidy the professors adhere to natural Selection —

Is any Review of Expiation coming with you? I wish you would just look at the letter taken — papers are returned (I wish?) —

Could you do me a great favour? Procure me a pamphlet published in 1850 in Charleston, entitled "Letter to J. Bachman on the Question of Hybridity in Animals by D. Maton". — Or was it in the pamphlet published in any Journal? I know his paper in Silliman (& post it is) —

53 Down Bowly Kent

My dear Gray

I was very glad to get your Photograph: I am expecting mine which I will send off as soon as it comes. It is an ugly affair, & I fear the fault does not lie with the Photographer. — I believe, but cannot swear, that I wrote & told you the Wright Review had come through Jarrold & Sons. I had time hardly to read it, before Hardy took it away. He much feared it was too general & not sufficiently historical enough for him. This was my impression, likewise; though I know it is very clever. What I shall do with it, if Hardy does not take it? I know no other Review to send it to. — I will write back I have had several letters full of highest commendation of your Essay: all agree, & fear the best thing that it is written, & I do not doubt it has done the Origin much good. I have not yet heard how it has sold. You will have seen Review in Chronicle. — There is to be a Review & U. Murray in next Friday's Phil. Journal. — I received the Letter of Credit returned: I am pleased & surprised at Profit from the American Edit. Remember that you are to be at no expense about your Essay: I personally nothing in literary, but sells in the Truth U. State. Poor dear Hamlin, to whom I owe much, is dying; & Horton is with him. In Many

for two set of sheet of your Proceedings. I cannot understand what Agassiz is driving at. — You once spoke, I think, of Prof. Bowen, as a very clever man. I should have thought him a simpleton, misbeliever & weak man from his writings. If you be agreed with me on any one point, I shall conclude that I must be in error on that. He never can have seen a herd of animals or he would see the difference of old & large dogs & young ones. — His paper about hybridizing beat everything. Tell a breeder that he might pick out his worst individual animals & breed from them & hope to win a prize; & he would think you had a fool, but mistake. — I believe Bowen is a metaphysician & that I personal accounts for an entire want of common sense.

Please remember Spiranthes; if you insert a column of grass, remember before you withdraw it to bend or bow it towards rostellum. Please if you come across wild Apocynum, observe whether it catches flies as in England. — I enclose my Photograph which has come rather crumpled, but I suppose can be ironed smooth. —

My dear Sir
Yours most truly
C. Darwin —

P.S. I enclose a little Photograph made by my eldest Son

Monday & my eldest Son

April 11th —

How time what you say about Melitta; but I will let them go. —

I have been idling & working at Primulas & think by experiment with explosive things Dimorphism: now I must work on price of information; I know the there are many cases of Dimorphic plants; but are not the two forms always borne on same plant? are there other cases of two forms living mingled in nearly equal numbers? — I have also been working on insect fertilisation of Orchids — beautiful facts — & I want ~~more~~ information on Cypripedium. Have you Botanic Garden? Could you collect up a plant with seeds & leave one uncovered; if it be one which sets seeds, & see whether protected ~~also~~ sets seeds, & whether the pollen of the two often intermixed

60

June 5th [1880]

Down.
Bromley.
Kent. S.E.

My dear Gray

I have been rather extra busy, so have been slack in answering your note of May 6th — I hope you have received long ago the 3rd Edit. of Origin. Asa Gray is an Entomologist & Horticulturist & now Secretary to Hort. Soc. of London. — He ~~wrote~~ read a long & hostile & rather weak Review of Origin at the Royal Soc. of Edinburgh. — I have heard nothing from Trübner of sale of your Essay; hence fear it has not been great; I want to say you must supply note. — I sent a copy to Sir J. Herschel; & in his new Edit. of his Physical Geography he has note on the origin of species, & agrees to certain

limited extent; but puts in a caution on design, so much like yours that I suspect it is borrowed. — I have been led to think much on this subject of late, & given to say that I come to differ much from you. It is not that designed variation matter, as it seems to me, my deity "Natural Selection" superfluous; but rather from judging lately domestic variation & seeing what an enormous field of undesigned variability there is ready for natural selection to select appropriate for any purpose useful to each creature. —

I thank you much for sending me your Review of Phillips. — I remember once telling you a list of trades which you ought to have followed; but now I am convinced that you are a born Reviewer. — By Jove how well & often you hit the nail on the head. You rank Phillips' book higher than I do; & there Lyell does, who thinks it fearfully tetsyede. — I amused myself & parodying Phillips argument as applied to domestic varieties; & you might thus prove that the Duck or Pigeon has not varied because the Goose has not, though more anciently domesticated, & no good reason can be assigned why it has not produced many varieties with respect to F. Water, shade, area, compared with sea & land, I believe ~~fully~~ when its play; in F. Water being been rate of change & of extinction, much slower, hence fossil fishes are all fresh-water. —

Bray 60

of time are in the same state.
Do not forget Spirentes — look
at flowers just opening: I am
curious to know whether I am
curious standard are in our
Spirentes. — But I suppose
you are also too overwhelmed
with public affairs to care
for science. — I have known
the Hemisphere so profoundly
interesting. The American does
not do England justice; I
have not seen or heard of
a book that is not with the work.
Tell me, pray, & I am sure, even
with the good, though at the
loss of millions of lives, that

to death would produce a crusade against Slavery. As to long run, a million horrid deaths would be amply repaid in the cause of humanity. — What wonderful times we live in. — Professor Agassiz seems to have with enthusiasm. Good God how I should like to see the greatest curse on earth Slavery abolished! Farewell. Hooker has been absorbed with poor dear revered Henslow's affairs —

Farewell

Ever yours

C. Darwin

How I wish like half-an-hours
conversation on Design; nothing
else would make our spirits
understand each other.
As no one has aided the
subject of natural selection
& to knowledge of my Book so
much as you, I must tell
you what has pleased me
much after the many attacks
on me for neglecting "Induction"
"Baconian philosophy" &c.
We in England think John
Stuart Mill the highest
authority on such subjects,
& he said lately & to a

Via Pl
July 21. 2. Hesketh Crescent
(We think to be here abt 4 weeks more)
Perth.

My dear Gray I regret
I have been indolent, otherwise
I wd. have thanked you heartily
for taking trouble about getting
me the 3 Hybrid pamphlets, which
I received just before starting
here for the sea for my
daughter's sake who improves a
little. — and now I have to
thank you for your pleasant &
useful note of July 20. —
I am now writing my Orchid
paper & am glad to hear what
you say about Cytisodium & that
you will have a look at Spiranthes:

remember that you must look at flower just opened & not visited & moth. I hope Hooker will send me an Aristolochia.

I am astonished at Dorsera filiformis mischeering: my experiments were all tried last in summer. Did you expect a rapid or visible movement? that my very takes place: the minutest atom of raw meat placed on a single gland, which is covered with viscid secretion, does motion best. — If gland is dry no movement takes place. I hope to finish my observations on Drosera this Autumn. From what I have seen of Cypripedium insigne my difficulty is to know what induces an insect to place its head so far back in flower: if you find many plants in flower watch for a little time for bare chance of an insect's visit. —

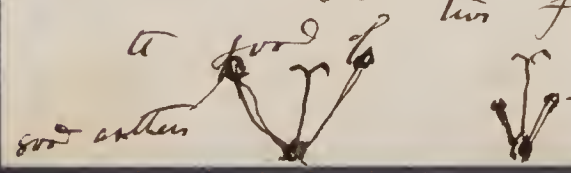
This diagram gives, as you know, the 2 forms of the stamens of Primula; not if you have of analogous cases, I shd. be particularly glad if you would name them & allow me to quote you: also if ever you have hardy plants & good seeders I would experiment on them. —


I cannot find the name given. —

[May 61]

for years you posterity would
blush the act. — But Heaven
knows why I trouble you
with my speculations; I
ought to stick to Orchids.
Huxley, I fear, is an usual
overtasking himself: I
seldom hear from him.
This place is charming; but
I am not as idle, as I
ought to be. Farewell
My good & kind friend
Yours most truly
C. Darwin

I am trying to make out
two forms of Thymus.




friend, who wrote to me, as
follows. "he considers that
"your reasoning throughout is in
"the most exact accordance
"with the third principles
"of logic. He also says
"the method of investigation
"you have followed in the
"of our paper to such
"a subject." — My wife's
remark on reading this, was
"why you know nothing about
(I mean with all my wife's sayings)
Logic". — Is it at any
satisfactory to me? —

I was very glad of your D.S.

on the state of your country;
on values a private note
for more than a dozen
public letters. After carefully
reading Olmsteads' last Book
I never doubted the North
could conquer the South.
But then what is to follow?
From Olmsted & Ruffelli letters
in Times, I cannot believe
that the South could ever
have fellow-feeling enough
with the North to allow
of government in common.
And the North endure a
Southern President? The whole
affair is a great misfortune
in the progress of the World;
but I do not regret it
so much, if I could persuade
myself that Slavery would
be annihilated. But
your president does not
even mention the word
in his address — I
sometimes think the contest
to give so desperate the
the North would be led
to declare freedom as
a diversion against
the Enemy. In 50 or

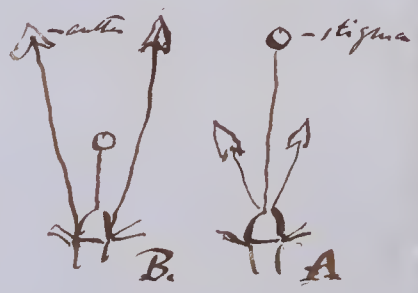
Sept. 16th

Down Bromley Kent

(4)

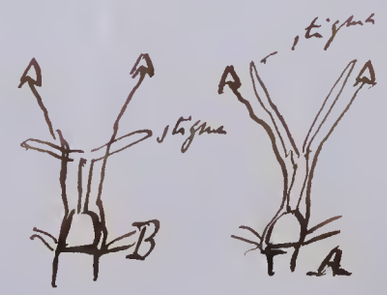
My dear Gray

In the whirl of your public affairs, science may be forgotten, or if not forgotten you may have no inclination to write. But if so inclined I sh. be very glad to have a little information on any issue of dimorphism, like that of Primula, & to be allowed to quote you. — To make sure that you may understand what I want to know; I give this beautiful diagram of the two forms of Primula.




I think I have made out their good or meaning clearly. The pollen of A is fitted for stigma of B & conversely. The individuals are divided into two sets or bodies, like the males & females

of Lucknow; but both in Primula
are hermaphrodite; but I will not
enter on details, as I will soon send
a paper to Linn. Soc. — I should
be eminently glad to know of
other analogous cases. Are the
two forms ever borne on same
plant? Thyme is a different case;
as the one form is simply
female, stamens being aborted;
the other female, however, I find to
be the most productive of seed.
Some of the species of Linum
often this case: I am
rather too late to experiment
fairly; but I am almost sure that pollen




of A is absolutely sterile on stigma of A; but
good for stigma of B: whereas pollen of
B is good for it on stigma B & for
stigma of A. — This infinitely interests me
much, so do help me if you can; for I
have some very faint hopes that it
may throw some light on Hybridization.
I have some seeds of Primulas raised
in very scanty numbers from stigmas
fertilized by what I call an homo=
=morphous union; i.e. of pollen from
plant of same form. —
If you have looked at Spiranthes let
me hear: but if your Spiranthes are
like S. speciosa figured by Bauer;
they are very different from our S. autumnalis;
which by the way at Torquay I had
the good luck to see. Bees

fertilisation: I have almost finished my
long Orchis paper, & then I must
go back to my tiresome work on
Cocks & Hens, Fowls & Rabbits.
Ehem Ehem, what much better
fun observing in these writings.
By the way I have just been
amusing myself in looking at
Dionaea in aid of my Drosera
work. How curious it is to see a
fly caught & how beautiful are the
adaptations compared with Drosera.
But I must not run on
about my hobby Horses. Farewell
My dear Gray
from my sincerely
Charles Darwin

you in to you in a most unceremonious way. —
I am so glad that you will look to some of your
Rubiaceæ, & I hope that you find time to make
a few experiments. — Thanks for notes about your Hollies,
& I hope you will look a little to them. There is to
me incomparably more interest in observing them in
writing; but I feel quite guilty in troubling a
~~them~~ subject, & not sticking to varieties of the com-
fanciers cocks, Hens & Ducks. — I hear that Bell is
savage at me. — I shall never resist Linnæus
next summer. — What you say about our keeping
~~in~~ out intercrossing & fixing long shots about
Design has made me laugh. — I suspect I am
more cowardly than you, as I ought to be, as
I do not feel sure of my ground. — Here is my
answering long shot about the cream-jug-notes:
I should believe it to have been designed (as I did
formerly each part of each animal) until I saw
a way of its being formed without design, & <u>at the</u>
<u>same time</u> saw in its whole structure (as in
homologies, embryology, rudimentary organs, distinctness &c)
evidence, of its being been produced in a quite
distinct manner ~~process~~, i.e. descent from another cream-jug

Intend means; i.e. descent

51a Gray

whose notes subscribed, perhaps, some quite heretical one, (6

when I think of yr beloved orchids, with rudiments of five ~~or~~ 3 anthers, with one pistil converted into a rostellum, with like the cohesion of parts, it really seems to me incredibly monstrous to look at an Orchid as created as we now see it. They last juvenile modification or modification. But enough & more than enough. – Farewell, my dear Gray, with cordial thanks for your never ending kindness. –

Your most sincerely C. Darwin

Of course I will send you my Orchid opuscules. –

Sept 17th 62 Read First Dear (1

My dear Gray. — I thank you sincerely for your very long & interesting letter, political & scientific, of Aug.st 27th & 29th & Sept. 2d, received this morning. — I agree with much of what you say & I hope to God we English are utterly wrong in doubting (1) whether the Rebs can conquer the S. (2) whether the R. has many friends in the South & (3) whether your noble men of Massachusetts are right in transferring your warm good feelings to the men of Washington. Again I say I hope to God we are wrong in doubting on these points. It is mainly (3) which alone causes England not to be enthusiastic with you. What it may be in Lancashire I know not, but in S. England cotton has nothing whatever to do with our doubts. — If abolition does follow with

your victory, to which would will look brighter in my eyes & in many eyes. It would be a great gain even to stop the spread of slavery into the Territories:— if this be possible without abolition, which I sh^d have doubted. You ought not to wonder so much at Enfield's address, when you remember the commencement of the war here many propositions were made to get things back to the N state with the N line of Latitude. — But enough of this: all I can say is the Massachusetts & the adjoining States have the full sympathy of every good man where I live; & this sympathy must be extended to other Federal states, if you ever have persuaded that your feelings were alienated to them. — But enough of this. It is out of my line, though I read every word of News & foreign with studied Avidity.

My other letter, ended, was unfortunately written last night: I send it because I am sure that when you understand what I want. [I have just looked at your "Manual" & see all that the color of the Rubiaceae is exactly the same as in Cinchona & Linnaea]. Did you know that in my case the pollen of the one form was not fitted to fertilize its own stigma? — that sterility ensued of about the same degree as when allied species are crossed? This fact seems to me so curious & very interesting. I sh^d be very grateful for any other cases in other Orders: I have found one Order

note of yours in which you say cases
abound in "Rubiaceae, Borragineae Labiatae
&c." Could you send me seed of any
such or Rubiaceae? I think these would be bad plants
to experiment on; yet I could to certain
extent. I was looking to Galium —
consider this summer & find many flowers
exclusively male: but I did not think of
looking to pollen of the hermaphrodite flowers.

I fear I shall weary you; but I
must write a little about Spiranthes.
At Torquay I was able to examine
(& cover up) growing plants; before I had
examined my cut flowers. In my note to
you I blundered somewhat. — I partly spoke
of the "cone" as being embedded within
the mentum; so it is in early bud, in so

for as that the back of "canoe" is covered with large cells with viscid matter, which burst & thus attach to pollinia, — a process which I could never before understand. — Just as you say, when flower first opens out a hair or needle can be passed down & thus (though straight) entirely remove the pollinia; in this early condition you will never find pollen on stigma; but after 24-48 hours, the Labellum moves a little away & these portions of nectar & oblique projecting stigma, allow the pollen-masses beautifully to strike stigma. — If pollen-masses have not been removed in early period, the bring of the proboscis is necessary for their removal. The Bee which I saw at work (one had 5 canoes stuck to proboscis, one on the other) alight at base of spike & go spirally

up it; when they get to upper flowers the
pollinia are attached to proboscis; they
then fly to another plant & always
alighting at base, they insert the pollinia
in the lower & more expanded flowers
& leave masses of pollen on stigmas
as I said. You do not seem to have
noticed the tingling of the foot of insection,
with the most delicate touch, which
seemed to me a vital action for
the requisite touch was too delicate
for mechanical action. Drosophyllum
I often saw that I tried D. rotundifolia to
often & showed the leaves to so many
persons, that there can be no mistake in my
observations. — For when there (I cannot say
how long without looking to my notes) after catching
a fly, the leaf cannot act. This is
independently of use to plant for which
the greater number of glands are dry,
usefully as a sticky like bit of moss or cinder
which has been clasped ~~in usefully~~
gets easily jerked or blown off. —
I hope to complete my work on Drosera;
but must act of virtue before it the
next year; otherwise I shall have,
with my small power of work, felt
as the volume of my longer
work finished. — I wish you could
see D. filiformis; I can hardly
believe that any Drosera does
not digest animal food. — I have
found best plants, is to try leaves
which have spread after plants

have been planted in Scrap-Plate.
Heaven forgive me, if you cannot, for
scribbling at such length. —

Your question what would convince me of
design in a poset. If I saw an
angel come from others to teach us good, & I was
convinced, from ~~that~~ seeing him, that I was
not mad, I shd believe in design. — If
I could be convinced through the life &
mind was in no unknown way a
function of the imponderable forces, I
shd be convinced. — If man was made
of stuff or vision & no way connected
with any other agencies which had
not lived, I shd perhaps be convinced.
But this is childish writing. —
I have lately been corresponding with
Spell, who, I think, adopts your

your idea of the theory of variation being
been led or designed. I have asked
him (& he says he will hereafter reflect &
answer me) whether he believes that the
shape of my nose was designed. If
he does, I have nothing more to say.
If not, seeing what Fanciers have done
by selecting individual differences in
the nasal bones of Pigeons, I
must think that it is illogical
to suppose that the variations, which
Nat. Selection, pursues for the good
of any being, have been designed.
But I know that I am in the same
sort of muddle (as I have said before)
as she to would seem to be in
with respect to free will, & yet they

supposed to have been foreseen or proclaimed.

Farewell my dear Gray, with many thanks for your interesting letter — Your unwearied correspondent

C. Darwin

Our English Holly (& all the allied species) are absolutely dioecious: I have just been looking at your Manual: but I cannot find out a fuller account of the state of some of the species ?? for instance whether the female flowers have any pollen (anthers but no pollen) & whether to make flowers ever set seeds. Our male trees never bear a berry. This would make a good case of gradation. — Henry showed me Holly years ago I have always felt curious about the steps by which it became dioecious.

Dec. 11th 1861 62

From Bowly Park

My dear Gray. —

Many & cordial thanks for your two last most valuable notes. — What a thing it is, that when you receive this we may be at war, & we two be bound, as good patriots, to hate each other, though I think I find this hating you very hard work. — How curious it is to see two countries, just like two angry & silly men, taking so opposite a view of the same transaction! So far as I can see we rest entirely on Wilkes' acting as judge. — I fear there is no shadow of doubt we have right, if the two Southern fogies are not given up. And what a wretched thing it will be, if we fight on side of slavery. No doubt it will be said we fight to get cotton; but I fully believe the thing has not entered into the mind in the least. — Well, thank

we private individuals, have nothing to do with
so awful a responsibility. — Again how curious
it is that you seem to think that you
can conquer the South; & I never meet a
Yankee, even those who would most wish it,
who thinks it possible, — that is to conquer
& retain it. I do not suppose the mass
of people in your country will believe it;
but I feel sure if we do go to war, it
will be with the utmost reluctance of all
classes, ministers of government & all. —
I am with you, & it is no use arguing or
thinking about it. — I called the other day
on Dr. Booth & was pleased to find him pretty
well & cheerful: I see, by the way, he talks
quite an English opinion of American
affairs, though an American in heart.
Buckle might write a chapter on opinion
being entirely dependent on Longitude!
Heartily thanks for Parl. on Dimorphism in Boraginaceae.

What a riddle it is to Mercurialis: I fear we shall
not be impossible to get seed. — It is my third of you
to try & send me the living plant of Hydrocotyle.
You may rely on it, I will send you a copy of
my Primula paper as soon as I can get one; but I
believe it will not be printed till April 1st.
& therefore after my Orchid Book. I care much
for your & Hooker's opinion thereof, & for the
rest of world, & for Bell's on Botanical points.
Bentham & Hooker thought well of my paper when
read; but no one can judge of evidence of
nature being a paper. The only thing which has
interested me of late is the making out that Lobelia
tri-dentata is made, more certain, with Jacobi,
& Nymphaea barbata the hermaphrodite, of the same
species: this is clear, & a country for all 3
forms appearing sometimes on same plant: they
differ as much as a pea-cock & peacen-hen. —
Bentham has given me list of species of Oxalis
dimorphous like Primula & some Menthas are so
too, — but I am not sure that he dis=
=tinguishes such cases as Hypnum. — I have not
yet had time to examine Cunninghamia, which you
sent me dried. —

If you can remember it, tell me when you write, how Dana is. — Hyde is going to publish, what will be an interesting little Book on the Jesuitical theology of man, & I believe he will touch on Spiritualism. —

With respect to Design, I feel more inclined to show a white flag than to fire my usual long-range shot. ~~When~~ I like to try & ask you a puzzling question, but when you return the compliment, I have great doubts whether it is a fair way of arguing. If anything is designed, certainly Man must be; our "inner consciousness" (though a false guide) tells me so; yet I cannot admit that man's indicatory mammae; bladder divided as if he went on all fours less; & pug-nose were designed. If I were to say that I believed this, I should believe it in same incredible manner as the orthodox believe the Trinity in Unity. — You say that you are in a haze;

Freq 62

I am in thick mud; - the orthodox would say in fetid abominable mud. I believe I am in much the same frame of mind as an old Gorilla would be in if set to learn the first book of Euclid. The old Gorilla would say it was of no manner of use; & I am much of the same mind; but I cannot keep out of the question. My dear Gray I have written a deal of nonsense.

Yours most cordially
C. Darwin

Jan. 22d, Down Bromley Kent. —

My dear Gray

Your letter interested us much; for we are all curious to see how things look to you all, & a letter is something living. — But first thanks for your next essay of Dimorphism: new cases are turning in almost daily, but I shall never have time to work a quarter of them. You will have received before this my Primula paper, & will know the amount of evidence. — I have been ill with influenza (indeed we do have, for ~~that~~ there have been 15 in bed in my household) & this has lost me 3 whole weeks, & delayed my little Orchid Book. — I fear that you expected in this opusculum much more than you will find — I look at it as a hobby-horse, which has given me great pleasure

to ride. I will write get flowers and send
them to-day if I can; for flowers, & painters,
often does not pick off the true color
in set up. — I shall be very curious
to hear what you think of it; for I
have no idea whether it has been worth
the trouble of getting up, — though to
fact, I am sure, it was worth my own
while in making notes. I am
heartily glad to hear a better account
of Dana, whom I much respect. What
a striking looking man! I forwarded
your letter to Booth & to Hooker,
from whom I had a long & capital
letter this morning. He is working
like a Horse. He is a good
fellow, by book or Not. Schuyler, he
says, has made him an aristocrat in
fact — he thinks breeding — the highest
breeding of the aristocracy — of the
highest importance. — Not for a few
words on politics; but we shall be few,
for we shall no longer agree, & class
& class, I shall never receive another
kind message from Mrs Gray. I
must own that the speeches & actions
recent of some leading men (I regard little
the newspapers), and especially the Boston
dinner have quite turned my stomach.
I refer to Wilkes being made a Hero
for boarding an unarmed vessel — to
the Judges advice to him — & to some
Governor triumphing at a shot being
fired, right or wrong, across the bows
of a British vessel. It is well to
make a clean breast of it at once;

& I have begun to think whether it would not be wise for the peace of the world, if you were split up into two or three nations. On the other hand I cannot bear the thought of the Slave-holders being triumphant; & it is really fearful to think of the difficulty of making a line of separation between the N. & the S., with armies, fortifications, & custom-houses without end with your retaliatory Tariff. Now I have done for myself in your eyes; & this grey with the indignant at having sent a kind message to so false a critic.

Well I can't help my change of opinion — It is all owing to ~~the~~ confounded Longitude —

Bad man, as you will think me, I shall always think of you with affection. — Here is an insult! I shall always think of you as an Englishman.

Ever yours very sincerely

Charles Darwin

P.S.

I have just performed an Herculean labour in looking through the nine big volumes of Lecoq's Bot. Geograph. — it is a horrid dull Book, but I have stumbled

on a few good facts; & on some
cases of dimorphism, — second in
Bottapinea & Labiatae — Lythrum
seems a very curious case for
the two or three kinds of flowers
occur on the same plant. — I am
now trying an experiment on
one of the Melastomas; & I
must suspect, that the two
sets of anthers have different
functions.?

Hottonia is dimorphic like
Primulas?

Kinnaeus & in the way I see
that I can find out whether the
plants are dimorphic in this
peculiar way, i.e. not in the
position of the parts & in its
relation to the two kinds of
pollen. — I am anxious about
this, because if it should
~~prove~~ so, it will show that
all plants with longer & shorter
or otherwise different anthers,
will have to be examined
for dimorphism. —
With you keep this note as a
memorandum? you see that I do
not scruple to trouble you. —
I am tired tonight, so will write
on two other subjects. —
Ever yours most truly
C. Darwin

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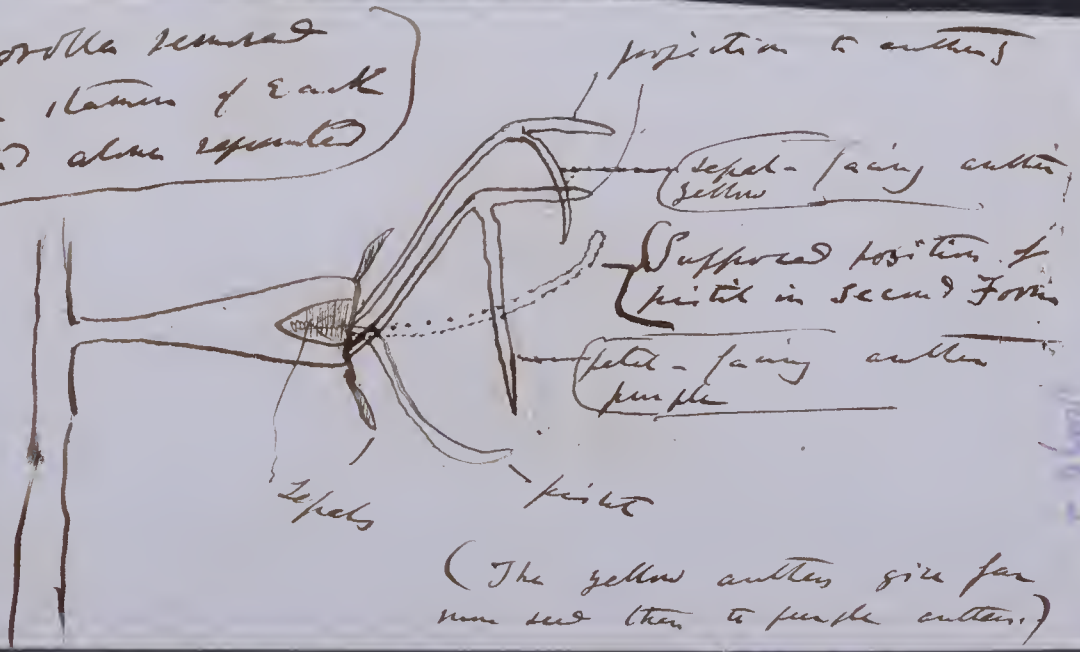
Feb. 16 Down Bromley Kent

My dear Gray
I have been trying a few
experiments on Melastomaceæ;
& they seem to indicate
that the pollen of the two
various sets of anthers (i.e. the
petal-faces & the sepal-faces)
have very different powers; &
it does not seem that the
difference is connected with
any tendency to abortion in
the one set. — Now I think I
can understand the structure
of flowers & means of
fertilization, if there be two

forms, — one with the pistil bent rectangularly out of the flower, & the other with it nearly straight. Study the enclosed magnificent diagram! Our hot-house & green-house plants have probably all descended from cuttings from a single plant of each species; so I can make out nothing from them. I applied in vain to Bentham & Hooker; but Oliver picked out some sentences from Naudin, which seem to indicate differences in the position of the pistil. I see that Rhexia grows in Massachusetts; & I suppose has two different sets of stamens. Now, if in your power, would you examine the position of the pistil in different plants, in lately-opened flowers of the same age (I specify this because in Monochoria I find great changes of position in the pistils & stamens, as flowers get old). Supposing the hypothesis should turn out right, please observe whether in both forms the passage into the flower is not in the upper side of the pistil, owing to the ~~lower~~ basal part of the pistil lying close to the ~~a~~ ring of filaments on the under side of the flower. — Also I should like to know the colour of the two sets of anthers. — This would take you off a few


Corolla removed
One stamen of each kind alone represented
projection to anthers
sepal-facing anther yellow
Supposed position of pistil in second Form
petal-facing anther purple
sepals
pistil
(The yellow anthers give far more seed than the purple anthers.)

in Amorphous, when they appear:
they will be very good, judging
from M.S. of the first chapter.
I wrote some little time ago
about Rhexia: since then
I have been carefully watching
& experimenting on another genus,
Monochætum; & I find, the
the pistil is first bent
rectangularly, (as in the sketch
sent) & then in a few
days becomes straight: the
stamens also move. If this
be not two forms of
Rhexia, will you compare
the position of the part in grey
& red flowers. I have

Down.
Bromley.
Kent. S.E.

March 15 1862

My dear Gray,
Thanks for the newspapers (though
they did contain digs at England)
& for your note of Feb. 18th.
It is really almost a pleasure
to receive stabs from so
smooth, polished & sharp a
dagger as your pen. — I
heartily wish I could sympathise
more fully with you, united
of course, hating the South.
We cannot enter into your
feelings: if Scotland were to
rebel, I persume we should

be my wrath, but I do not think we should care a penny what other nations thought. The millennium must come before nations love each other; but try & do not hate me. Think of me, if you will, as a poor blinded fool. I fear the Greatest state of affairs must dull your interest in Science. —

Two days ago, I heard from Trübner, who says of the 250 copies of your Pamphlet, he has only 38 in hand; so that he will, I suppose, soon transmit to you a few pounds, enough to cover all your expenses. I believe the pamphlet has done my book great good; & I think you from my heart for yourself; & believing that the views are in large part true, I must think that you have done natural science a good turn. Natural Selection, seems to be making a little progress in England & on the Continent; a new German Edition is called for & a French one has just appeared. There has even been a Dutch Edition! One of the best men, though the present unknown, who has taken up these views, is Mr Bates; my old friend Mr Wallace

suspicion (perhaps with the
proud worry when seed-capsules
are ripe) that one set of
anthers are adapted to pertic
in early state, & the other
set for it in its later
state. — If Ben visit the
Alexia, for Heaven's sake
watch exactly how the anthers
& stigma behave, both in old
& young flowers... & give me a sketch.

I have got lots of seeds & plants
for experiments this summer,
including Commelina specialissima!
again I say, do not bother
me. Even you must buy
me.

C. Darwin

to gradation — seen in your
Hollies. — So far as I can judge,
I am not very wrong, but dis-
-tinctly wrong about heterostyles,
at at least about some of
them: if a Rhexia grew in
a garden, it would be good to
cover up a plant under net
& see if it seeded as well as
unscreened plants. —
Thanks for Müller's pamphlet, this
is very good & I had not seen
it; indeed I see hardly any
Reviews or Periodicals. —
Hooker has been here for 3 days & we
had lots of pleasant talk: I am
always fond of discussing & learning
for him: I wish he had not 10
Treasures & Dig a pit in hand, as the
Genera Plantarum. — Your affectionate
C. Darwin

Down Bromley Kent
Sept. 21

My dear Gray. — I am in
your debt for two pleasant
notes. First for business: I
should greatly prefer your not
returning any of Fritzen's communications;
but you really must not return
more than half, or otherwise
I shall have gained an inverse
advantage in having given away
~~several~~ many copies of your Pamphlet,
gratuitously. So add to all
your kindness of letting me keep
them as long as I am. I have
settled with Fritzen. — Fritzen
~~secondly~~ has not sold quite all;
& copies are quite separately tied

I shd certainly like ½ a dozen or
dozen to give away. I was
asked for one last Saturday.
I have never made one form
who was not delighted with
your writing.?

Secondly, in a week or 10 days
I shall send ½ dozen of my
on Otaheite,
as you desired; & the half
will soon follow for my set of
the whole, before my printed off
sheet. — I fear it can never
be popular; but do not guide
too much by first half: for, if I do not
decide myself the two last
chapters are better. — I believe
I have been very frank in
publishing in popular form. —
When I told Murray that I wanted
them sheet to send you; he thought
of some arrangement for American
republication, as he said Lyell's new
Book is to appear in America;
but with my incomparably less
important book it seems to
me, as things now are, quite
out of the question: but I have
thought it as well just to
mention what Murray said. —
The North seems going on
steadily victorious; & thank God
there is distinct ground both for
on to Slavery question; but we
stupid English cannot yet
believe that you will ever
be a single Union again. —
I hope that you will ask your
pupils to look carefully

June 10th. 86 Down Bromley Kent

My dear Gray.

Your generous sympathy makes you overestimate what you have read of my orchid Book. But your letter of Aug. 18th + 26th has given me an almost foolish amount of satisfaction. The subject interested me, I know, beyond its real ~~no~~ value; but I had lately got to think that I had made myself a complete fool of publishing in a semi-popular form. Now I think confidently deny to work. I have heard that Bentham & Oliver approve of it; but I have heard to opinion of no one else, & that opinion is worth a farthing. Which strange creatures these orchids are, for instance Monomeles, of which I have this morning examined another species, & which supports all that I have said, but which has completely puzzled me.

I thank you most heartily for your note on some American species. I am not surprised, as no true Orchis grows near you, that the pollinia of O. spectabilis were not removed. I ought to expect that it would take probably a long time before new insects would learn to dodge. — You probably pushed too hard against the viscid disc & crumpled the enclosing sheath of membrane, which, I know, interferes with the proper movement. I will write to Murray about cost of 3 first words out; but I doubt whether he will send to costs, for I believe that there is to be note to be sent to Germany for German edition. — I will do my best, but I fear you should not pay for them. If there be (which is very improbable) an American Edt, Murray will expect a little more than simple cost. But I will keep back this letter till I hear from him.

Enough & too much about my orchids, which are now again become beloved in my eyes, & which were quite lately accursed. Many thanks about copies of your Pamphlets. Do not trouble about Hollies; I thought they grew near; the case is not important. Nothing will be made out, I fear, about Rlexia, unless indeed a plant or plants cut be protected from insects. I have now a Rlexia glandulosa under trial, but there is little difference in flowers & little to be made out. I am working at sweet Melastomas; but am at fault; I am, however, certain there is something very remarkable; the pollen of one set of

others produce less seed & by amongst
these seedlings are dwarfs compared to the
other set, all produced from the same
plant. – The labour is great: I have
lately counted over & over 6700 seeds of
Nemophila atomaria! Mr Meehan has
sent me his paper on parallel differences
in trees of N. America & Europe: pray
be so kind as to remember to tell me
whether this can be approximately trusted;
for the case is interesting enough, as
best case I have seen of apparently direct
action of conditions of life. – Forgive
me for one bit more trouble: I have a
Boy with the collecting mania &
it has taken to poor form of
collecting Portugese stamps: he is terribly

Eager for "Wells, Fargo & Co Pony Express"
"2d & 4d stamp" & in a lower degree ~~the~~
"Bloods' 1. Penny Envelope, 1, 3, & 10"
"cents". If you will make here this
present you will give my dear little
man as much pleasure, as a new
& curious genus gives us Rd Society.

Since this was written the dear little man
has been struck down with scarlet-fever; but
thank God this morning the case has taken
a mild form. — I have just received
your long letter on Lycophidia; you may
believe how profoundly interesting they are to me.
Will you not publish them, either in notices of
Boa in Silliman, or Ibis? But your notes
are more interesting than you will suppose, for
while publishing I saw at Flower show. C. hirsu-
-tissimum, but could not track it, but it seemed
to me that the strike author entirely correct

Gray 66

the passages of the author. I was amazed & I am clear that there must be some strict distinct manner of fertilization. But I did not think of insects carrying into flowers; this life of different kind of pollen & in somewhat uncertain & varied stigma. But how it is wonderful. You have hit on the same very idea which Lettsom has overpowered me, viz the exuberance of contrivances for same object: you will find this point discussed & attempted to be partly explained in the last chapter. No doubt my volume contains much error: how curiously difficult it is, to be accurate, though I try my utmost. You note have been interested me beyond measure. I can now afford to d-d. my critics with indefatable complacency of mind. Cordial thanks for this benefit. — It is ma==ddening to me that you have strength of mind to care for science, amidst the awful events daily occurring in your country. I daily

look at the Times with almost as much interest as an American could do. When we peace comes: it is painful to think of the desolation of large parts of your magnificent country; & also the speechless misery inflicted on many. I hope & think it not unlikely that an English war may conclude that it will take a long time for prosperity to return to you. It is an awful subject to reflect on. — Good Bye my dear friend. — I will keep this open till I hear from Murray, which I think must be tomorrow.

Why keeping back this letter till I hear from Murray, do, I fear is absurd. I have now received your interesting notes of June 2d. How can you write whether your letters bore me? I never in my life received a letter from you that was dull. Your letters are a very great pleasure & profit. I seldom see or hear from a soul on Science. Most of my scientific friends

[See p. 8 on back of p. 5.]

[This page has got in wrong place]. (8

are so busy that I scruple to write to them.
Arthur's is very pretty: I think congratulation its felicitous
was effected nearly as described under Cattleya;
for it seems to be with Vanilla, which I have
lately seen. How rude you are attending to
Cypripediums. I can at any time return you
(making up in advt) your notes on this genus
or others: notes. How very very kind it is in you,
overwhelmed as you are, to send me so many
notes. — Hearty thanks about Hounslowia: that
subject, I am working at hard & intensely
now much. — By the way did you see Cook
at the Linn (so-called inspected) flower of
Vanda & Oncidium: it was very curious, the
pollen-grains emit their tubes which within
the cellular: it is curious to see them tubes
travelling up in straight lines from the broken
cellular in Oncidium, right to stigmas: it is like

I shall be glad to see you finding their way to orchids.

I received 2 or 3 days ago a French translation of the Origin of Species by Mad.lle Rojer, who must be one of the cleverest & ardent women in Europe: in ~~every~~ spirit & hates Christianity, & declares that natural selection & the struggle for life will explain the morality, religion of man, politics & so on !!! She makes some very curious & bits, & says she shall publish a book on other subjects; & a strange production it will be. Good Bye — this I hear from the watch-house. (I have had another look at Vanilla & from Arethusa; stamens very like Vanilla & unlike the other orchids. In Vanilla, the labellum is furnished with a compound curious comb, which would compel an insect in retreating to rub it back against anthers; but the papillae in Arethusa seem very different.

How beautifully clear the spirals & dots are visible in writings of Chinese, Persian & Arabian.

If you will keep Specularia to look & tell me whether pollen-grains emit tubes direct from anthers or grains collected on collecting hairs. —

I have just had letter from Alp. De Candolle about Primula, & he gives me facts & his opinion show he appreciates the case, & about nat. selection. He says he goes as far as you about change of species, & he laughs at Linnæan definition "Species ..... quot creavit". — But I told numerous gent. sent creatures. — But I think from his letter you go further; he says he wants direct proof of nat. selection & he will have to wait a long time for that. Opticians do not wait for direct proof of undulations of ether. But good Heaven what a higgledy-piggledy letter I am scribbling to you, who have hardly a minute to spare. It is a horrid shame. 10 I will stop. 20th At last I have heard from Murray that he will instantly send to 3 authors & publishers, if wanted; a publisher has also set "on easy terms" with him, if wanted; yours affectly C. Darwin

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July 1st Down Bromley Kent

My dear Gray

I have been badish for 2 or 3 weeks, but am better ~~today~~ to-night & mean to amuse myself (& have no other pleasure) by scribbling a few lines on orchids. Since writing last, I have received your notes on Platanthera Hookeri & on diversity of forms of Cypripedium. On the latter you might, if you can spare time, write to write a paper. Platanthera Hookeri is really beautiful & quite a new case. It is almost laughable the vivid lines getting 10 feet apart that the front of the flower has to be divided into two bridal chambers!

I have added a note to the German Edition about this & a few words on Cypripedium on your authority. On getting your letter I wrote to Trübner to send the ½ dozen copies: I wish you would let me pay for them; let you can 10 functions that you would fling without perception first greeted the Orchids in the 1 can shakespeare manner in which you did the F. Fly 1 on George, who is an enthusiast, has been watching orchids with enthusiasm & indefatigable patience. He has made out clearly that it is a Fly, (Empis) which fertilises O. maculata, & probably the Diptera fertilise the chief other species: it was pretty to see the pollinia affixed to their spherical eyes, & after the act of deposition parallel to & either above the proboscis. But the most remarkable case is that of Hermidium Mormochis: he has brought me 24 specimens of very minute Hymenoptera with pollinia attached to them, & clings to the same exactly spot, viz. the posterior base of femur of front legs. Nothing has given me such an idea of the adaptation of form of orchid flowers: the labellum hangs obliquely downwards & the minute insects enter between the

Edge & the large veins dive on
one side; it is retaining they but their
prominent fences against the under
side of the disc. So closely filled is
the flower to the insect, that my
son saw several times insects
after entering in a wrong position, can
not, change their position & secretion.
So much for orchids: I am fairly as-
=tonished at the success of my book (not
that I know whether it sells) with
Bentham: Berkeley has received it
in Sir R. rather sparingly, & Hooker writes so strongly. So that
I can dismiss the subject & stick to other work. Lately I

been doing very little, except some copying of plants.
I have made a good series of experiments on
the petaloid flowers of Pelargonium: but I
doubt whether I shall get such good results,
as I at first hoped with respect to
sterility of hybrids. By the way Naudin
writes that he is going to publish on this
subject this autumn: his paper gives me the
idea that he does not know what has been
done in Germany. — Petunia glandulosa, I

have required insects acting to set seed; but I see no sense in the probability of dimorphism. You mentioned some genus (name forgotten without searching your letter) in which you found two forms like Primula, & a third form with both pistils & stamens short. Can you tell me whether the flowers on the specimen were then characterised? I should wish to know because Lythrum is trimorphic —

Yours very truly
C. Darwin

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July 14' Down Bromley Kent

My dear Gray

I shall have returned the enclosed some; but we have had a miscellaneous lot of days. [the Postage Stamp collector] Our Boy has had a return of Scarlet fever, with all sorts of mischief, kidneys, glands of neck &c & still has much fever. But he is now made up for night & I will try & forget the misery of this wretched world, & write a bit about science. — With respect to Pogonia, it would be a very great anomaly, if insects open to attract for nectar: give lay nothing about to intellection: for Vanilla I shd. expect the acid matter

would be fixed under lids of antlers. Insects
ought to be watched at work. The Australian
Bees open the indusium of Lindsaeaceae
for pollen — Platanthera fimbriata
is a pretty case. There is no end
to the adaptations. Ought not these
cases to make us very cautious
when we doubt about to use
of all parts? — I feel believe the
the structure of all irregular
flowers is governed in relation
to insects: insects are to Lords
of the flora (to quote the witty
Athenaeum) world. — How will you have
worked the N. American Orchids!

I am heartily glad of the time the
you have spent Lecture are over.
Hooker is very anxious about Mrs Hooker
& her started on health-tour to
Switzerland. Today I heard of the
sudden death of poor Dr Fel. Brown,
just as he finished translating my
Orchid-book. There is misery &
anxiety everywhere. — Poor dear Hooker
seemed very anxious. —
A few days ago I made an
observation which has surprised me more
than it ought to do — it will
have to be repeated several times,
but I have scarcely a doubt of its
accuracy. — I stated in my Primula

paper that the long-styled form of Primula grandiflora was utterly sterile with own pollen; & I have lately been putting the ~~two~~ pollen of the two forms on the division of the stigma of the same flower; & it strikes me as very wonderful, that the stigma distinguishes the pollen; & is penetrated by the tubes of the same & not by those of the other form (which is the same thing; nor are the tubes arrested.) The stigma of the same form acts on & is acted by (for the papillae become colourless) pollen, which produces not the least effect on the stigma of the other form. Taking prepotent power as the criterion of difference of the two forms may be said to be sexually distinct. — Farewell my dear friend

C. Darwin

Down Bromley Kent

Sept 23d

My dear Gray — I received several days ago two large packets, but not yet read my own letter; for we have been in great distress & I not allowed to attend to anything. Our poor Boy had the rare case of second rash & sore throat, besides mischief in kidneys: & as if this was not enough a most serious attack of Erysipelas without typhoid symptoms. I despaired of his life; but this evening he has rather improved & I think he will live. He has lived on Port-wine every 3/4 of an hour day & night. This evening to our astonishment he asked to see his stamps were safe & I told him I to one sent by you, & then he wd. see it tomorrow. He answered "I should awfully like to see it now": so with difficulty he opened his eyelids & glanced at it & with a sight of satisfaction said "all right!" — Children are our greatest happiness, but often & often a little greater misery. A man of science might be lower, perhaps not a wife; for then there would be nothing in this world worth caring for & a man might (whether he would in another question) walk away like a Trojan — I hope in a few days to get my Brains in order & then I will pick out the your Orchid letter (& read by & by your last) & return

them in hopes of your making use of them — Plantation there would be entirely well worth giving & as much as feel safe about Lycopodium: in fact I am not sure that I understand the passages of which insects crack in & out. Could you give a Diagram? I have had an access of letters & such a number of experiments, all going to the dogs, that I have not time to make abstract of your letters. Will you return me such, as you do not use: but I hope you will be able to use all some time or another. — I shall be very glad to hear of Rossmaeler's* observations & Hunter's & you just alluded to them. — You did formerly talk about Infusoria: in Vista & Cycles the case seem to me to be much too remarkable to be called precocious flowering!

* I hope he will publish notes; I hear the French say the very paper on Primula is also pure imagination; but I cannot hear that this is founded on any observations

You will never read my horrid writing, if I write on both sides of the paper, which I have taken in balance to address. — Of all the carpenters for knocking the right nail on the head, you are to my best: his one idea has provided the very chief intent in my wicked book, has been that it was a "flank movement" on the enemy. I live in such solitude that I hear nothing, & have no idea to what you allude about Bentham & the existence of Species. But I must confess. — By the way one of my chief enemies (the side on which has arrived on) namely Owen, I have his been lecturing on Birds & admits that all have descended from one, & the advocates his own idea that the oceanic weighty Birds have lost their wings by gradual disuse. He never alludes to me or my with bitter sneers & coupled with Buffon, & the Vestiges. — With it has been an amusement to me this first evening scribbling as egotistically as usual about myself & my doings; so you must forgive me, as I know with your kind heart you will do. — I have managed to skim the News paper, but had not heart to read all the bloody details. Good God what will the end be; perhaps we are too dependent here; but I must think you are too hopeful on your side of the water. I never believed the "canard" of the army of the Potomac being capitulated. My good dear wife & self are come to wish for Peace at any price,

Good night my good friend. I will write to
no more — C. D.

Chr. mm wd. I shd like to hear what you
think about the Pedigree in last Ch. of Orch.
Book on the ~~[illegible]~~ cause of the extinction
of varieties, & means for varied general
purpose. It bears on Design & that early
question — Good night & good night.

P.S. Last night after writing the above, I read to get bundle of notes. Little did I think what I had to read. What admirable observations! You have delivered me on my own hobby-horse! I have not had for weeks such a glow of pleasure as your ~~letter~~ observations gave me. — Plat. hyperborea is indeed a most curious case & especially interesting to me. How like to Bee Ophrys. Does it use in arctic regions when insects may be scarce? It would be very good to ascertain whether there is any occasional cross-fertilization in this species. How curious about the necklace. See my note p. 324 about Acacias. Acacias, I am sure, bees, also, must chiefly visit to reach A. hirsuta. How organic beings are connected! How excellently you have worked Cyp. spectabile. I doubt if my fertilization may always be of simple insects altogether wrong, & fertilization may always be of simple insects bodily crawling in: I will you could get some 2 quarts to water on warm day for 2 or 3 hours a fine sheet of some Cypripedium. What density in Platanthera. Your observations seem to me much too good to be sunk in any review of my book: they will be noticed. — But I am so very sorry I did not return your M.S. earlier: I shall be so grieved if I have caused you inconvenience: but in truth it was physically impossible for me before last night to read a letter to anything. Farewell my good Friend

C. Darwin

Feb 28 Down Bromley Kent

My dear Gray. — I hardly know what to thank you first. Your stamps gave infinite satisfaction. I took him first on the 2 then an hour afterwards another lot. He actually raised himself on one elbow to look at them. It was the first animation he showed. He said only "You must thank Prof. Gray awfully". — In evening after long silence, then came out the oracular sentence "He is awfully kind". And indeed you are; overwhelmed as you are, to take so much trouble for one poor dear little man. — And now I must begin the "awfullys" on my own account: what a capital notice you have published on the Orchids! it could not have been better; but I fear that you overrate it. I am very sure that I had not the least idea that you or anyone would approve / do so much. — I return your last note for chance of your publishing any notice on subject; but after all perhaps you may not think it worth while; yet in my judgment several of your facts, especially P. hyperborea are much too good to be merged in a Review. But I have always noted that you are prodigal in originality in your Reviews. — I will with all my heart I could understand one of the common vibratile labellum in foreign orchids: insect would have to watched to work. —

You speak of two Pogonias always growing together; see my account

of Asparagus, a mere acid. Cannot you set a pupil to watch Catopsis?
I shd. be very much surprised if insect suck stigma: the speculation
crossed my mind & was rejected for want of any analogous case.
I received two copies of your Review; so I sent one to Hooker,
as if you have not sent it, he would much like to see your
excellent remarks. — You exactly express my conclusion about
Greenland. Hooker, I think, underrates occasional means of
transport. I'll I proved to contrary, he used to maintain
vehemently instead: a fact that to see would kill old Lyell.
I have not had time yet to look at Mitchellia. Cannot you
persuade your pupil to publish underline not & experiment
on some plants. — Perhaps Hottonia is visited by butterflies: I
have reason to suspect that many Galiaceae are so visited.
I was looking at Lythrum salicaria this morning: it is
beautifully dimorphic like Primula, but with addition in
both forms (I believe there is a third form) of six short
stamens: it will be curious to make out use of them.
But I must shut head, otherwise I shall spend my life over
dimorphism: what I said about pollens & stigmas of Linum
grandiflorum, having each other in confound & further experiment,
I have written to Trimen about to copies of Orchid Book.
Farewell. This is a dull letter & is sent merely to give
you my most cordial thanks. — Adios

C. Darwin

Aug 9 Down Bromley Kent

My dear Gray — It is late at night & I am going to write briefly & of course to beg a favour. But first let me thank you most cordially for the stamps in letter of July 21, of <u>inexpressible</u> value to my Boy, who has gloated over them once — We hope in bed-carriage to get him to see a Wednesday next. — I have settled with Trübner; I possess he spoke to Murray for him who charged me reduced price. — If you have spare copy pray present one from me to your friend (<u>Rosbach?</u>)

6/8, instead 19/

The Mitchella very good, but pollen apparently sterile. I have just received Hottonia good difference in pollen. — Echium vulgare a humbug, merely coarse like Thymus. But I am almost stark staring mad over Lythrum; if I can prove what I feel believe, it is grand case of Trimorphism with 3 distinct pollens & 3 stigmas: I have counted & footed above 90 flowers, typy all 18 distinct crosses which an epithet within limits of this one species! I cannot explain, but I feel sure you will think it grand case. — I have been writing to Bennett to see if I spiff can get L. hyssopifolia, & it has just flashed on me that you might have Lythrum in h. & I have looked to your manual for the flora of Northern America. — Have a look at some of your species, & if you can get me seed, do; I want much to try species with few stamens, if they are dimorphic: I see verticillata I'd expect to be trimorphic. Seed! Seed! Seed!

I wd. write when read
of Mitchell — But she
forbidden! —

Your ever obliged and friend

C. Darwin

Then = reason in my mind,
for I can see that to those
who already believe in
change of species, their facts
will modify to certain extent
their view (from ~~Exclusive~~ the heterostylic nature)
Homomorphic grandchildren of Primula
can become more sterile. —

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Aug 21. Southampton

My dear Gray.

We are a wretched family & ought to be exterminated. We kept here to rest our poor Boy on his journey to Bournemouth, & my poor dear wife sickened with Scarlet-fever & has had it pretty sharply, but is recovering ~~pretty~~ well. Our Boy suffered badly from the journey, though we took it on the advice of two Doctors. I fear he will be an invalid for months, if not years, —

There is no end of trouble in this wicked world. — I shall not feel safe till we are all at home together, & then the devil be I know not. But it is foolish complaining. I received a few days ago your letter of Dec. 4th with all the interesting details on Hymenoptera. It seems a grand case. I hope that Mr Rothneck will send public them. The simple fact of two patterns in the same species, & the reciprocal action of two hermaphrodites

seems to me well worth establishing;
& till my first account is
confirmed, nothing can be
considered as established.
I feel no sort of doubt
after repeating my experiment
on Primula; but I shall
probably not publish till winter,
(even if then) & to Mr. R.
could I first establish the case.
I must just recur to stamps:
my little man has calculated
that he will now have six
stamps which we offer Boy in
to school has. Then in a triumph.
Your last letter was plastered
with many coloured stamps &
he long surveyed to envelope

4/

in bed with mind quiet satisfac=
=tion. I wrote you a mad
letter the other day about
Lythrum; but the case is worth
your madness. Thanks for
remarks about Rhexia; what
you say about pollen flitting
out agrees with what I have
seen. — My Rhexia glandulosa
seems very different, & I believe
offers nothing odd. Heterostylism &
this, I suspect, turns out, as I
suspected, something marvellous like
Lythrum. — I know <u>almost</u> as
well as you, that systematic
work is the foundation of
<u>everything</u>; get in your case
& Hooker's case, I perpetually

feel inclined to do an
systematic work. — I had a
note from Hooker this morning
giving a pretty fair account
of Mrs Hooker; but it almost
seems that her heart is
slightly affected. He tells
me that he has got two
wonderful different flowers or
same spike of a Vanda.
Huxley is going to bring out
a very curious Book on
Man & Monkey. —
I can see no Harm in
materialism; but secretion of
Harm depends on most delicate

combination of circumstances. If
I come I hope will go on for
many days & secret way & then
with increasing alt commence —
I am writing away at a
great rate.

Affairs seem to be getting
with you more & more
terrible. What will to end
be. It seem to me been
far more fearful, than it
apparently does to you.
Farewell my dear friend.
C. Darwin

I do very much like, if time
permits to hear what you think of
my last chapter in Orchid-book. —

& of modified petals, like coloring;
& then the colored petals belong
to an inner whorl & are
modified stamens. By the way
can you tell me of any flowers,
with petals uniform of different
colours; I believe that this would
be pretty sure guide to hibernation
or thermophilism. — All my semi=
botanical work, as you know, has been
connected with insects, & now I am
almost sure [but I find it a disgusting
truth that with me first observations are
generally all a blunder] the flowers have
led me to a curious little discovery
with respect to the bee-keeper's friend
in the wild, the Hive Bee. I saw
the other day to my dismay (see Origin)
Hive Bees sucking to common red
clover, but it was a second crop, which
I am told produces sterile flowers;
but many of the Bees were attempting
this, but always inserted their heads

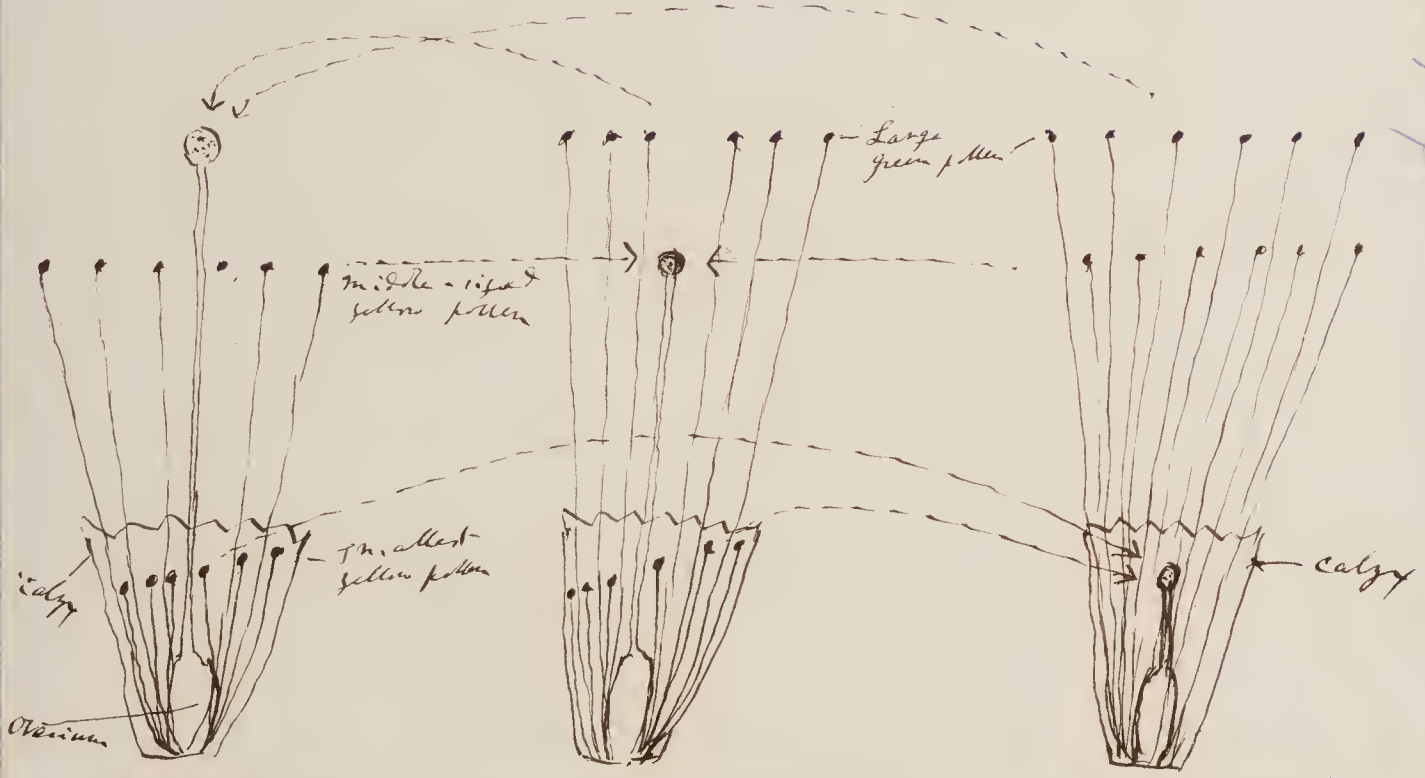
Cliff Cottage. Bournemouth. Hants
My dear Gray. I have just received
your most kind letter of Aug. 18 & 19
& will answer myself, now my patients
are in bed, by beginning a letter to
you. My poor Boy (whose warm face
blushes up to the eyes at the thought
of writing to a live Professor) has
this day made a marked step & has
taken several walks of a few hundred
yards; & my wife is recovering well &
her skin really peeling. — We have
taken two houses here, so I hope & trust
this dreadful fever will not
spread. — I am very glad that at
present you intend to publish some
separate notes on Orchids, which you
have so capitally worked at. The Gentian
I am not a fair judge; but I think
that it is worth your while. — I am
pleased to hear about Fodgetia & about
Gymnadenia tridentata. Your account makes
me think the latter like the Bonatea

specious & often & often have I speculated
what on Earth could be meaning of
the wonderful horn-like stigmas & projecting
anthers. I suspect its stamens may have
been arrived at by a process somewhat
analogous to that which apparently has produced
the wonderful mechanism of Euphrasia
sesquipedalis. It would appear
that self-fertilisation is commoner
than I thought: since publishing I
have found that Nertia ridiga-
-ris fertilises itself, if insects
fail to do the job — many
thanks for Houstonia seed. —
I am glad to hear, but disappointed,
about the Spectularia pollen-tubes.
I cannot resist sending you a
diagram about Lythrum [why should
I resist? does not prescription
give a right? & have I not
for long years written & bothered
you about all sorts of things!] Lythrum
has 3 kinds of stigmas & 3 kinds of
pollen; the stamens of same height
on two of the three forms producing the
same sort of pollen, & I cannot
doubt are fitted to fertilise the
stigma of that height. I conclude
so from watching the Bees; but
hope to prove it by crosses. So the
two here three hermaphrodite forms
each depending on half the stamens
of either one of the two other
forms. This is thus of the utmost
importance. This strikes me as a
very curious case. The three forms
occur in about equal numbers.
By the way it seems to me that
Lindley in Veg. Kingdom describes the
homologic structure of flowers wrongly:
the so called calyx, with its 12
hundred of spirals sepals, appears
to consist of 6 narrow sepals

between the flowers & sucked at holes
bitten through the corolla. – The same
bee always followed the same practice.
And apparently those which suck
at the mouth of the flowers have
a longer proboscis than the other
bees, which suck through the holes.
Farewell my dear Gray. Forgive
the scribbling about my own work,
for I seldom see a soul to talk
with on natural history.
Good night – Even your mind must
C. Darwin

Since writing the above my first I
have found I have an much astonished
blunderhead about the proboscis;
but if you had seen the Bees, the
blunder was almost excusable –
What an ass I was to
scribble all the above. –

Lythrum salicaria



Progress of Education — One of my little Boys, Horace, said to me, "There are a terrible number of adders here; but if everyone killed as many — they could, they would sting less!" — I answered "Of course they would be fewer" Horace "Of course, but I did not mean that; what I meant was, that the more timid adders, which

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Pen away & do not sting
would be saved, &
after a time none
of the drones would
sting". —

Natural selection!!

Oct. 18 1911 Down Bromley Kent

My dear Gray

I have not indulged myself for some little time in writing to you, though I have to thank you for two very pleasant notes, & for some pages of Silliman with several notices which I was glad to see. Lythrum salicaria is coming out so clear (though the seed not counted) that I do not care much for other species; but I shall be very glad of seed of Nesaea. My geese are always at first swans; but I cannot help going on marvelling at Lythrum. It is a consolation to me for I am utterly routed, beaten, "skipped" by these odious melastomaceæ; yet I feel sure there is something very curious to be made out about them. On getting your

last note, I looked at Rothamsted observations
on Hordeum; they are copied on the
structure; but when I began to think
how I could put the case for
fund. Chem., I failed; for the observations (fertilization)
are not sufficient about reciprocity.
It would be a pity to spoil a good
case: if said a few experiments, if
spare seed will grow, I could make
a paper & give all his facts.
Now that we are at home again, I
have begun dull steady work on
"Variation under Domestication"; but also
& also following wild plants is much
better sport. — By the way at Bournemouth,
for the want of something else to
do, I worked a bit at my old
friend Drosera: I took to testing all sorts of
fluids, which are not corrosive & do not,
I believe, act on ordinary organic
compounds, but do act on the nervous
system of animals; & I declare I
am coming to the conclusion that plants
or at least Drosera, must have something
closely analogous to nervous matter. It
was pretty to see effect of acetate of
strychnine, how it stopped the movements,
how acetate of morphia greatly delayed
& retarded movement. I think I shall
some day pursue this subject. —
Another little point has interested me,
viz. finding such a number of natural
hybrids between two species of Verbascum;
& linking V. thapsus & lychnitis
closely together. They are all utterly sterile.

this fact has given Hooker, & when I
told it, a fit of the horrors. —
So we are all come to see
next summer! Alas, my days for
moving anywhere are come to an
end. Many thanks for sending the
article in Daily News, which we
read aloud in family conclave.
Our verdict was, that the N. was fully
justified in going & was with the S.; that
the moment it was plain that there
was no majority in the S. for Reform,
you right, after your victories in Kentucky &
Tennessee, to have made peace &
agreed to a divorce. How curious it
is that you all seem to believe
that you can answer the facts; whereas
on this side of the Atlantic, it is the

almost universal opinion that this is utterly impossible. If I could believe that your President proclamation would have any effect, it would make a great sensation in my mind; I would then run to think of your seizing on Canada (I will write also my heart it was an independent country) & declaring war against us. But slavery seems to me to grow a more hopeless curse. How detestably the special correspondent of the Times writes on this subject: the man has not a shade of feeling against slavery. This war of yours, however it may end, is a fearful evil to the whole world;

but will effect wile, I must think,
be felt for years. — I can see
already it has produced wide
spread feeling in favour of
aristocracy & monarchism: no one
in England will speak for years
in favour of the people governing
themselves. Well good night. —
Do not be indignant with me
& do not let Mrs Gray be
more indignant than she can
help. — Good night & farewell.

Yours cordially
(Hort. Soc. June 1st)
C. Darwin

N.B. Do you chance know anything of Mr Fly of N. York who sent in 1846 to Fridly 2 ears of wild maize & says he cultivated it & saw Brazil decreasing. Delaville Sutton says. Do he [illegible] write to Mr Fly?]

more gorgeous than ever, comes
on sorrow, & that is an comfort.
I am glad to hear that you have
sent off your account of Achidy
to Newbery; let me have
a copy if you can, for I see
no periodicals. — I wish you had an
active profit in the country: it would
be curious to Rock up with
a smelling to bites on each side of the
stile outlet in Cyprididae;
& then if fellow were at all
disturbed it would show that
little insect had entered of the
toe. I shd be very glad indeed
of Mitchelle, & of seed, if possible,
of Nelsoa. I am more than

interested in Lythrum;


Nov. 6 From Bromley Kent

My dear Gray

When your note of Oct 4th & 13th, (chiefly about Max Muller) arrived I was
nearly at the end of the same book & had in=
=tended recommending you to read
it! I quite agree that it is
extremely interesting, but the latter part
about first origin of language
much the least satisfactory.
It is a marvellous problem.
I have heard, whether true or
not I do not know, but the
book has rather given me the
same impression, that he is
dreadfully afraid of not being
thought strictly orthodox. He even
hints at truth of Tower of Babel!

I think accounted for certain sneers at me, which he seems to get the better of towards the close of the book. — I cannot quite see how it will forward "my cause" as you call it; but I can see how anyone with literary talent (I do not feel up to it) could make great use of the religious in illustration. What pretty metaphors you would make from it! I wish some one would keep a lot of the most noisy monkeys, half free, & study their methods of communication!

A book has just appeared here, which will, I suppose make a noise, by Bishop Colenso, who, judging from extracts, thrashes most of the Old Testament. — Talking of Books, I am in middle of one which pleases me, though it is very innocent food. Its "Miss Cooper's Journal of Scientific!!" Who is she? She seems a very clever woman & gives a capital account of the battle between wit & grim words. Does it not hurt your feelings that we thrash you so confoundedly. I am sure No!! Grey will stick up for grim words. Ask her whether they are not more honest straight good sort of words. — The Book gives an extremely pretty picture of one of grim villages; let I see grim autumn, though so much

May 78

the seed of my 88 cirsiums flowers from
truth of his agreement, if you
remember it; but there is something
more, mid-July but is in addition
moderately fertile with help of
own stamens, & I must make
my mind expire, & shall not
publish this year. The case, I
think, is worth any labourer. —
I did not know, & yet doubt,
that it was Hooker who reviewed
me so gorgeously in Gardeners
Ch.[n]. but I have asked him. —

Now for two questions: please
give me <u>reference</u> to your
notice of grounds affecting each
other point; & secondly on the
movements of the tendrils. —

Also do you know of any medical description of the gum vars. of Maize, & if so give me title. — & if I fail to get them, could you help me to half-a-dozen grains of the most marked varieties of Maize. What a good book "Dressing on Fruit" is. —

I am crawling about today & have been compiling all about preachers & declaimers; & a curious case it is. —

I'd like to try a few experiments on gum Tendrils: I wonder what would be good & easy plant to raise in pots. Farewell, my dear Gray. God help your poor country, though perhaps you serve out pity. Farewell, my good friend

C. Darwin

49

Nov. 23d

From Bromley Kent

My dear Gray

I have been rather slack in doing my boy Leonard's instructions to thank you most sincerely for a number of stamps with which your envelope of Oct. 27th was plastered [the endorsement was duly posted] & for a precious Canada stamp previously sent. He believes he is my far the richest boy in the whole school in the American stamps, & expect to get wonderful treasures by trading with those sent by you in duplicate. The Resda seeds came safe, & are we get a treasure to us as the stamps to Leonard, & stranger I cannot put it. I see you say you have sent 2d article on Orchid Book to Gillmore: I have not received the first, which I th. they must be out, & which I th. very much like to perhaps. By the way in the last Edinburgh Rev there is an article by Duke of Argyll on "Supernaturalism" in which Orchids are brought in: it is clever; but I do not see that it really removes any of the difficulties of

of Theology. — In the last Macmillan there is a little
on Reviews on Max Müller, — on the origin of language;
(by my Butter-in-law, H. Wedgwood & his daughter)
which I think is worth looking at. This letter
is a sort of Literary Intelligencer; for I am
going to tell you that Bates' paper on Butterflies
in last Nat. Hist. Linn. Transactions
of Amazonia, is well worth some labour in
studying, that art of your special line. — The
mimetic cases are really wonderful, & he has
brought to clearly before my mind the process of segregation
of varieties into species. But I don't whether you will have
time to go into paper carefully enough to appreciate it.
Bell's book, which I am very anxious to see is not sold
out; but nearly 1st 4000! copies the other day at
his auction.
I have nearly finished a long chapter on the simple
facts of the variation of a few of our cultivated
plants & I ought to be very much obliged for any
answer to one question. Is the fruit of a Fragaria
Virginiana much larger, (twice or thrice?) than that of
F. vesca? and secondly do you know anything of the F.
grandiflora, said to have come from Surinam, & often
called the Carolina strawberry. How far does south from F.
virginiana range?
Do you remember about my Boy, Horace, on the natural
selection of coward adders? I must tell you that
the other day he overheard me talking about species;
& afterwards he came to me, with his eyes fair
with astonishment & asked "Did people formerly really
believe that animals & plants never changed? I
answered he yes. "Well then what did they say
about the kinds of cabbages & peas in the Garden!"
I answered that these were all due to man's
agency. "But do not wild plants vary?"
I answered that they varied within certain fixed
but unknown limits. So this lad struggled his brilliant
with pity for the poor people who formerly believed
in such ~~things~~ absurdities. — I believe Horace
is a prophetic type, an Agassiz could say, I
future naturalist?
I see in Times today the great news of Macdelland's
finishing — Good God what will be the end of all! —
Given my dear Frog
Yours most truly
C. Darwin

Nov. 26

From Bonly Park

My dear Gray.

The very day after my last letter from of Nov. 10th & the Review in Silliman, which I feared might have been lost, reached me. We were all very much interested by the political part of your letter: in some odd way one never feels that infl= =uence & opinions printed in a newspaper come from a living source; they seem dead, whereas all that you write is full of life. — Many thanks for P.S. about maize; if the basket from had been the aboriginal, it would very have at variance to reality: there must be some mistake in statement of Indian, quoted by Aug. St. Hilaire. — The Reviews interested me profoundly: you rarely ask for my opinion & you must con= =sequently endure a long letter. First for Dimorphism: I do not at present like the term "Diœcio-Dimorphism"; for I think it gives quite false notion, that the phenomena are connected with a separation of the sexes. Certainly in Primula there is unequal fertility in the two forms, & I suspect this in Primula sapon in case with Linum; & therefore I felt bound, to state that it might be a step towards diœcious condition; but I believe there are no diœcious forms in Primulaceæ except a Lysimachia. But the three forms in Lythrum contain no Diœcia. But the three forms in Lythrum contain no Diœcia is in no way necessarily connected with the two phenomena

with any tendency to separation of sexes. The case seems to me in result a function to be almost identical with what Mr Crk. Sprengel called "dichogamy"; & which is so frequent in long heterostylic groups; namely the pollen & stigma of each flower being mature at different periods. If I am right it is very advisable not to use term "diœcious" as this at once brings in idea of separation of sexes. — I hope you will be able to attend a little to Plantago: I can hardly understand the sentence in your article, in which form does stigma popid in bud (this occurs in long-styled Lythrum, but is not true for Plantago)? is the short-styled (i.e. your long-stamened) really sterile? You will think that I am in the most unpleasant, contradictory, fractious humour, when I tell you that I do not like your term of "precocious fertilisation" for your second class of dimorphism. If I can trust my memory, the state of cradle, of stigma & pollen-grains in different parts in state of bud; that they are in a condition of special modification. But upon my life I am ashamed of myself to differ so much from my betters on this head. — The temporary theory which I have formed on the class of dimorphism, just to guide experiment, is that the perfect flowers can only be perfectly fertilised by insects & are in this case abundantly crossed; but that the flowers are not always, especially in early spring, visited enough by insects, & therefore the little imperfect self-fertilising flowers are developed to ensure a sufficiency of seed for parent generations. Vide Primula in sterile, when not visited by insects, but then so visited from plenty of seed. I infer from structure of 3 & 4 forms of ~~Trapa~~ Balsaminea that they require insects; at least there is almost as plain adaptation to insects as in Orchids. — I have Oxalis acetosella ready in pots for experiment next spring; & I fear this will upset my little theory; unless I can as Hooker says "shake you with wriggling out of anything." — Compositæ Campanulaceæ, as I found this season, is absolutely sterile if insects are excluded. Specularia speculum is fairly fertile when enclosed; & this seemed to me to be partially effected by the frequent closing of the flower; the inward of course expansion folds, corresponding with the cleft of the open stigma, & in this action pushing pollen from outside of stigma on to its surface. Now can you tell me, does Spec. perfoliata close its flower like S. speculum with inward expansion folds; if so, I am smashed without the fearful "wriggling." — Are the imperfect flowers of your Specularia the early or the later ones? very early or very late? It is rather pretty to see imitation of closing of flowers of Sp. speculum. — I entirely agree with you in your remarks on the part which cross-fertilisation plays. I was much pleased by Oliver's remarks in Dr. Hooker's Review of the Primulas case, on the lower plants having sexes more often separated than in the higher plants, — so exactly to reason of what takes place in animals. — Hooker in Review of Orchids repeats this remark. There seems to me much truth in what you say, & it did not occur to me, about the improbability of Hermaphroditism in certain lines in long organised beings. I could hardly doubt that the

Hermaphrodite state is the aboriginal one. But how is it in the conjugation of Conferva — is not one of the two individuals here in fact male & the other female ?? I have been much puzzled by this contrast in sexual arrangements between plants & animals. Can there be anything in following consideration. By roughest calculation about 1/3 of British genera of aquatic plants belong to Linnean classes of Mono- & Diœcia; whilst of terrestrial plants (the aquatic genera being excluded) only $\frac{1}{13}$ of genera belong to these two classes. Is there any truth in this fact generally? Can aquatic plants, being confined to a small area & small community of individuals, receive more free crossing? But to return to our point, does not Alph. Decandolle say that aquatic plants taken as a whole are less organised compared with terrestrial; & may not Oliver's remark on separation of sexes in less organised plants stand in close relation to their being frequently aquatic? Or is this all rubbish?]

[I have left myself little room for orchids & indeed I have little to say except to express my admiration at cleanness & ingenuity with which you explain & describe all the forms. It seems to me all excellently done, & has interested me beyond measure. — Do you Platanthera smell sweetest at night; then I suspect in close guide the moths are the fertilisers. — I have been especially interested of case of P. psycodes, more especially

nice to D. of Argyll's interpretation remarks on the case of
Angraecum, which in action seems analogous to your case.
But I fear the most wonderful is the case of Sr
Tridentata; I hope you will confirm the remarkable
a physiological fact. If I understand rightly the
rostellum alone is penetrated, — the part primarily
of a stigmatic nature. In this observation you have
anticipated an experiment, which I mean to try, namely
pollen-tubes will penetrate the great rostellum of
Catasetum — I dare say you are quite right about
self-fertilization being much commoner than I thought
with orchids. Did I tell you that I have found in
Mattia nidus avis that the ovaries, if in course of
few days the flowers are not visited by insects?
Your observations on Cypripedium seem excellent; &
I dare say I am wholly wrong; it seems to me more
likely the small insects should lick juice off the
hairy petal-joints or that pubescence, than with long proboscis.
How curious about the little bristles on the stigma!
What a magnificent compliment you and your
Reviews will! You & Hooker seem determined to turn
my head with conceit & vanity (if not

deeply turned) & makes me an inbeacoble wretch. —

With most cordial thanks, my good & kind friend

Farewell

C. Darwin

P.S.

In my last letter, I mentioned ~~Walter~~ Bates' paper: he is a man of high origin, of great force of character, & wonderfully self-educated, but constitutionally of low vitality & poor & under unpleasant circumstances of life. Could you induce any of your Zoological co-editors, just to notice his paper (& if so inform me): it would be a good & charitable deed, for it would encourage & please a man, that wants & deserves encouragement.

What a fearfully long letter I have written!

P.S. 2. Would you be so kind as to tell me whether Fragaria vesca & V. virginiana differ much B.dorsically for I cannot make out the exp. you have succeeded in copying them. —

I have just had long letter from Hooker on part of that curious plant in Nature; I must consider it well, & see if it alters my notions. —

in Dimorphic & have written to
Thwaites in Ceylon to try to
follow. — I will send my Linnean
paper whenever published. —
I was heartily glad to receive
a note a week or 10 ago
from Dana, giving a moderately
good account of himself. —
I thank you most heartily
for all your extraordinary
kindness in helping Leonard
so much in his perfection
for stamps. He has just
exchanged one of Blood's
for, I believe, 9 rare
stamps! I, also, of late

Gray, 56

Jan 2d. Down Bromley Kent

My dear Gray

I have been rather extra muddle
of late & overburdened with letters,
but I cannot rest without
thanking you for your two notes
of Nov. 24th & Dec. 9th (the former
with answers to several queries)
& especially I must thank you
for Box with plants (I wrote
& thanked Capt. Anderson) which
arrived in first-rate order
& are planted. Pritchard to
Mitchella looked as fresh, as if

bug up to day before! What a pretty little creeper it is with its scarlet berries. Hooker, I remember, mentions it. — I hope to Cypripediums with flowers & I have been thinking how I will try them, viz. by putting a live mouse in & flopping up end of thistles & catching them as they come out of labelled orifices, & then, if they are smeared with pollen, I will put them in a pan & so make them go to work & then examine stigmas. —

I have just finished paper for Linn. Soc. on Dimorphism of Linum, — much better case than Primula: I see Planchon says the L. Leonii (var. of perenne as he calls it, I doubt not falsely) bears on same plant flowers with long, & short & equal (to anthers) pistils; I wish I could get seed of that arctic plant; I should like to see that new case. — I have Ornithogalum growing wild in my greenhouse & from Mediterranean; so I think I have as good a stock as I want in this Family. I have Cinchona

anyone in U. States can care
for science & persevere —
I am building a small stove
house, so that if I have
strength I shall have better
means for my little experiments.
Ever yours very truly
C. Darwin

If a flower of an Oak or Beech
had fine green
well-shaped corolla & calyx, would
it be the closest analogy
in vegetable kingdom!
This query, I daresay, shows
my profound ignorance. —

troubled you with an extra
number of questions. —
It seems quite strange that I
have not the trifle to
ask you tonight, if you
can remember it, viz. to weigh
in grains one of your wild
Fragaria virginiana. —
Have you ever attended much
to garden plants; if you
have ever noticed any
what some gardeners call
sports & what I shall call
"bud-variations", I shd be
glad of an opportunity to add to my

large collection of Poets,
which seems to me of value
in regard to theory of —
versatility. Hooker is in
great spirits at having
finished Wolsitrchia & is
going to Paris. — Good
night my good & very kind
friend, I am tired. I
fear the last has been a
graceful battle & defeat.
When will peace come? But
then Slavery. I know not
what to wish. I wish to
Heaven the North did not
hate us so, I, for one, could
wish them heartily for you
then; even though I doubt the
war being our justification.
But thank Heaven wishes
make no difference. Dr.
Boott seems in despair &
hates to hear of all the
bloodshed. We in North
England seem tiding over
our difficulty for better
the cotton seem centered to
hope. The subscriptions have
been gigantic. Good night
again. I cannot help
still wondering the sun has

Jan 5/19th. Down Bromley Kent

My dear Gray

When I look over your letter of Dec 29th & see all the things you tell me & all the trouble which I have caused you, overwhelmed as you are, upon my life I am ashamed of myself. — I will be less unreasonable for the future; but with lots of gold close beneath the surface it is hard not to dig for it. I was glad to hear of Platanthera & to Butterfly, & hearty thanks for the Indian corn; what little grains! I know nothing about "glucose partly replacing starch". I have several odds & ends to say: Bates has forwarded copy of his paper to you for Hooker. I have received

for next Nat. Hist Review, & five pages will
therein, if you care, give the cream of
the case. — Do not trouble yourself
to weigh, — I asked, for fruit of wild strawberry;
what you say suffices for my purpose. —
I was muddled about 1st part of your
Review of orchids — it is not worth explaining how I
came to be so; but my memory returned
the day after my letter went; I confounded
one Review with another. —
You ask about "Dichogamy", term & fact are
given by C. K. Sprengel in his Entdeckte
Geheimniss der Natur, — a curious old
book full of truth with some little
nonsense. — I have been thinking how
interesting it would be to experiment on the
3 kinds of flowers of Linum Lewisii, but I fear
it would be impossible to get seed. — I have
been at those confounded Melastomas again;
throwing good money (i.e. time) after bad.
Do you remember telling me you did see no
nectar in your Rhexia: well I can find
none in Monochætum, & Baker tells me
that the flowers are in the most marked
manner neglected by Bees & Lepidoptera in
Abyssinia. Now the anthers projecting, or horns to the
petals of Monochætum are full of fluid, &
the impression occurs to me the Diptera
or a small hymenoptera may puncture these
horns like the puncture (proved since my
orchid book was published) to dry nectaries
of true Orchids. — I forget whether Rhexia
is common; but I very much wish you
would kept some watch on a warm day

a group of flowers & see whether they are written
& think whether & what they do. — I think
I die a happier man, if I could understand
these Melastomas. Oh dear, I wish
poor worried Mr Rotwick would not care
for such trifles as the welfare of his
country, & would stick to flowers!
Well, your President has issued his Proclamation
against Slavery — God grant it may
have some effect. — I fear it is true
that very many English do not now
really care about Slavery: I heard some
sensible people saying here the same thing;
& they accounted for it (& such a contrast
it is to what I remember in my Boyhood)
by the present generation never having been
a head made about Slavery. —

I sometimes cannot help taking most gloomy view about your future. I look to your money depreciating so much that there will be mutiny with your soldiers & quarrels between the different states which are to pay. In that anarchy & then the South & Slavery will be triumphant. But I hope my dismal prophecies will be as utterly wrong as most of my other prophecies have been. But everyone's prophecies have been wrong; those of your Government as wrong as any. — It is a

cannot still to the other world;
I hope that you may
prove right & good come
out of it. — Do not trouble
or tire yourself to write
to me, — though I never
receive a letter from you
without real pleasure &
much instruction. —

Farewell

Your most sincerely

C. Darwin

Feb. 28° 55 Down Bromley Kent

My dear Gray

Many thanks for your note of Jan 27th. The Enclosures were forwarded. The Marquis seemed very pleased a treasure; for besides seeing the kind, a young man at Edinburgh with experiments on the mutual fertility of some of the varieties. — Pray think, when you see, Dr Sudden about Pelicia (from whom I have since received a pamphlet): there was very good remark about attachment despite of two to eye or proboscis; & these are only two parts where I have seen attachment. Thanks, also, about higbush & berries of Oak-tree: Hooker was pleased (to whom I mentioned your remark) about the "Commissariat" of Plants. — When I send my Linnean paper

paper you will see about L. Lewisian —

If you have time I think you will be interested in a part of Lyell's Book on Man: but I fear the latter part, about flint implements, may be too geological for anyone except a regular geologist. He quotes you at end with quotes. By the way he told me the other day how pleased you had been of hearing that the Ty could pardon your pamphlet. The "Pall Mall" also speaks of it as the ablest contribution to the literature of the subject. It delight me when I see your work appreciated.

The Lyells came here this day week, & I shall question at his explicit caution: I feel sure that he doubts almost fully the modification of species by variation & selection; & yet, though writing at length on the subject, he is afraid to say so; & he will not serve as guide to anyone. The public may well say, if such a man doubts, how can we who are ignorant, & from even a great one subject. — Lyell was pleased, then I told him lately that you thought the language might be used as an excellent illustration of the modification of species; you will see that he has a admirable chapter on this —

I received a letter a little while ago the correspondence between Fitzroy & Fitch: I could take you he is he interested us all: it is so real; & it seems to see two able & honest men suffering so enormously. Of course I side chiefly with the Englishman; but I have so

well understood your horror of disturbance.
It is very natural that you shd. dread
becoming split up like Germany; but to us
it does not seem quite so horrible.
I think both correspondents underrate the
very general belief entertained for many
years in England, that your Government
delighted in making us eat lies, &
that we had eat lies about Bundey
Line, Right of Search, Vancouver &c &c.
I believe that this has greatly checked
all sympathy with you; & made the
whole country fitter up, when, as we
thought, you had played us swindling
tricks in the Trent affair. — after
finishing the above correspondence; I
read Carlyle's excellent Lecture, which shows
10 well how you stumble across the Slavery.

It made me for a time wish heartily for north; but I could never help, though I tried, all the time thinking how you, if he be bullied & forced into a war for you, then you were triumphant. But I do, must truly think it dreadful that the South, with its accursed Slavery, shd triumph, & spread the evil. I think if I had power, said thank God I have not, I would let you, compass the border states, & the West of Mississippi & then force you to admit the Cotton States.

For Do you not will begin to doubt whether you can conquer & hold them? I have inflicted a long tirade on you. — The Times is getting more detestable, — but that is too weak a word, — than ever. My good wife wishes to give it up: but I tell her that is a pitch of heroism, to which only a woman is equal to. To give up the "Bloody Old Times" as Abbott used to call it, would be to give up meat drink & air. —

Farewell my dear Fox

Your most truly

C. Darwin

interesting letter from Dr. Crueger of Bot. Gardens of Trinidad, & he tells me odd fact of native species (& of native species) of Cattleya &c which never open their flowers, & get set seed-capsules. Happy man he has actually seen crowds of Bees flying round Catasetum with the pollinia sticking to their backs! I wrote to him to ask him to observe what insects did in flowers of Melastomaceae: he says not proper season yet, but that on one species a stray Bee seemed busy about the

March 20th Down Bromley Kent

My dear Gray. I have very little to say, but will amuse myself by scribbling a few lines to thank you for information in last note of Feb. 9th & to thank you in my dear little man's name for two precious stamps. He told me with joyful triumph that in American stamps he equalled all other collections put together in the school. He exchanged a duplicate Blood's stamp for a whole lot of treasures. He says there is an envelope of same value on the stamps you put on your letters, which

would be of value to him. —
I have one request to make
for myself, viz. seed of Campanula
perfoliata: I have tried in vain at
Kew & elsewhere for some. —
I am very glad you like Bates'
paper: I expect his Amazonian
travels will be good. If you
read Spettigue's book, tell me what
you think of it: I (& Hooker)
have told him that we regret
much that he did not speak
more boldly at about Species.
He answers that his belief in
changes fluctuates. — His Book has
made me recant. You ought
I think it as much as ever.
What a dead hand you are in
passing a lounge & transfixing
your adversary! You ask about
Sprengel's "Dichogamy": he means by
this a plant in which each flower
first matures & sheds its pollen & then
later its stigma matures; & much
more rarely matures its stigma
first & subsequently its pollen: it is
that these plants are in function
monoecious. I am sure his statement
is to large extent correct, & the
case is very common. In the
Iris-like order the plants
are in function diœcious. —
A couple of days ago I had an

horn-like appendages to the anthers. It will be too good luck if my study of the flowers in the greenhouse has led me to the right interpretation of these queer appendages. By the way, I have just built a hot-house & got some orchids, & it amuses me much. — Some plants of Amaryllis aulica specialis, at least the seed was so named (shade dark orange flower, elongated hairy leaves) have just begun to flower, & I find in two plants that the stigma stands on a raised level with anthers;

Mary Ward to Darwin
Chapter on Wind & Insects
Insect pollination

hence I fear they cannot be dimorphic. — Your Mitchelles look healthy: I hope they will not flower very soon; for my health (& that of my Gardener Boy) has been of late so bad, that we have resolved all to go about middle of April for 6 or 8 weeks to Malvern for Water Cure for me & change for my Boy. — It breaks my heart: I shall never get my present Book on Variation under Domestication finished; yet it interests me much & I am now in middle of long chapter on Inheritance Reversion &c, giving results of my own & other Breeders' Experiments. — Good night. —

My dear Gray

Yours most truly

C. Darwin

April 20 51 Down Bromley Kent

My dear Gray. — Thanks for two notes, one about the Duke of A. (did I tell you how capitally that was criticised in the Saturday Review?) & the other note very savage against England: I cannot help fearing that we shall drift into war: what a curse it will be to us anyhow; for you seem to be getting to like it. — I wish I had known when I read the correspondence that Mr Loring was your father-in-law: I sh. have read it, if possible, with still greater interest. We must keep to science, I fear, for we both seem to be getting to think each other's country conduct worse & worse. But I sh. like to know whether General Butler is in your honest opinion a bad man. —

Your remarks from Wyman about the Incas came

very appropriately for I am at present running up the facts on this subject & give facts on both sides. — I now regret that I have always intentionally evaded case of man; but have put in a note on such facts as I have heard of. — I sent off two days ago two copies of my Linnean paper: I hope to see my interest you: it has me. Monochaetia turns out with me only variability in length of pistil. — I forgot to ask about Intermarriage: ~~Darwin~~ M. DeVay explicitly that State of Ohio, from relations, has legislated against cousin marriage: can this be true? Here is another heterogeneous question: have you ever formed any theory, why in spite of horses (I have been reading your most clear account in your Lectures) the angles go $\frac{1}{2}$ $\frac{1}{3}$ $\frac{2}{3}$ $\frac{3}{8}$ &c — Why should there not be $\frac{1}{4}$ or $\frac{1}{5}$? It seems to me most marvellous. There must be some explanation. If you have theory, I know it would be too long to explain; but I'd like to hear whether you have. My good friend Falconer has been writing to me that these angles go on as fixed a law as that of gravity & never vary. I can fancy the packing of organs in very early parts of the embryo & consequent interruption in the flow in the spiral. Speaking of Falconer, I was very sorry to see his letter in the Athenaeum; so vituperative & violent towards Spell. We have had lately sharp sparring in the Athenaeum. Did you see the article on

Heterogeny or Spontaneous generation, written
I believe, certainly by Owen!! it was
in Review on Carpenter, who seems to
have been silly enough to Owen calling
one Carpenter's master: it was like his
clever malignity. Under the cloak of
a fling at Heterogeny I have sent a
letter to Athenaeum in defence of
myself, & I take only advantage to
quote Spectator <u>amended</u> verdict on the
Origin. — I suppose my letter will
appear next week: it is no great
thing. — Be sure, if you have
opportunity read Bates' Amazonia,
because you will like & admire
it. —

I have got to say a few words on Orchids,

I hope that you may have time to look at the Rostellum of Gymnadenia, this summer; because a good deal is in Bot. Garden at Edinburgh, a Mr J. Scott has been experimenting on foreign ~~species~~ genera, & he finds that the rostellum stimulates some kinds to protrude their tubes; but the tubes never penetrate the rostellum only creep along its surface to the stigma. He wrote to me to know whether I could give him any details on your observations. By the way I lately found some fungoid with 3 pistils, but united, so that I could peep into the ovarium: I put in pollen & afterwards found the tubes expected & attached to & apparently penetrating the ovules; but never of the micropyle! I have now no doubt that you are perfectly right about fertilisation of Cypripedium: a friend lent me a plant of C. pubescens. I put exceedingly minute Bee (an Andrena) into the Labellum & covered orifice with wet paper; but this precaution I afterwards found superfluous, for all the edges of the orifice of labellum are folded over (just like insect traps for ~~opening~~ in Sundew Kit-cats) so that the Bee could not crawl out. — Well the bee immediately crawled out & ate out. —

of the windows opposite the anthers, with his back towards the anthers, against which he, forcing, pressed it, owing to the elastic work opposite the anther. It was pretty to see under lens how to while thorax & base of wings was smeared with pollen. I put bees back into the labellum five times & five times I saw his back smeared. As you know he must pass under the stigma (with its spines directed towards the apex as you describe) for there is no other passage, & as I expected when I cut open the flower I found the stigma well smeared with pollen. — It was beautiful. — Good night

My dear Grey

Ever yours cordially

Though an Englishman

Charles Darwin

My Nr [illegible], 57 Down Beckly Kent

My dear Gray

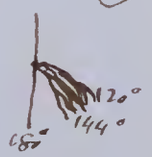
I have to thank you for 2 or 3 little notes. The last I was glad to receive on Bell, & will tell him, when I write, what you say on Species-portion. I am pleased at it; but cannot quite agree. You speak of Bell as a Judge; now what I complain of is that he declines to be Judge. It puts me into despair, when I see such men as Bell & you in equipoise (as you think) of deciding: I have sometimes almost wished that Bell had pronounced against me. When I say "me", I mean only <u>change of Species & descent</u>. That seems to me the turning point.

<u>Personally</u>, of course, I care much about

Natural Selection; but this seems to me utterly unimportant compared to question of Direction

Mr Mad-Direction. — Like a hunger Afs I have written two stupid letters to Athenæum: the letter to above effect. — How clever & original & candid your remarks about Language & Design. —

Your little discussion on Angles of Divergence of leaves in a Spiral has almost driven me mad. My 2d Boy George is a good mathematician, & when I showed him the fractions, he said they formed a converging series; & I see clearly pretended, they do all consist round one point. I have been drawing all to real angles & correct angles on a spiral, & I see the angles which do not occur in nature, are just as symmetrical in position as the real angles. If you wish to save me from a miserable death, do tell me why the angles of $\frac{1}{2}$ $\frac{1}{3}$ $\frac{2}{5}$ $\frac{3}{8}$ do series occur, & no other angles. — It is enough to drive the quietest man mad. — Did you & some mathematician publish some paper on subject; Hooker says you did. Where is it? I have been visiting for a fortnight house of relation to try to get a little health for my youngest Boy (the Natural Selection Hero) & self; with very poor success. This has led me to muddle my brains over the angles of leaves. — Do you know of any plant in which angle is fluctuating a variable? I often blow science; for when drawing I forget my discomfort & do no other time am I completely for two successive hours. — Remembering your Tolerants I have been



looking at Plantago lanceolata — it is a
dichogamous
Female Dichogamous — (i.e. pistils mature & fertilised
long before anthers of same flower mature)
; fertilised by the wind; and a few plants
have imperfect anthers, containing little
pollen & a part of this imperfection —
amygdalinic
Euphorbia I find, is also a female dichogamous
monoecious plant, & which is diœcious in
function at any one period. But why I
bother you with these trifling facts, I know
not, except that I have nothing to do, & vanity
to you makes me forget by one obvious selfishness —
Farewell my kind & good friend. If you can
spare a copy, send me one on De Candolle.
Farewell — yours most truly

C. Darwin

Aug 31 From Bromley Kent

My dear Gray. I was very glad to receive your Review of December a week ago. It seems to me excellent & you speak out, I think, more plainly in favour of derivation of species, than hitherto, though doubtfully about natural selection. Great to point, I am easy about to second. Do you not consider such cases as all the Orchids next thing to a demonstration against Hooker's view of species arising independently & monstrosities: it is impossible to imagine 10 very coadaptations being formed all of a chance blow. Of course Creationists would cut the enigma. What an inimitable worker you are! Why these Reviews, supposing I were to attempt them, would take me a month's work.

I have written twice to you lately ago, & sent 2 copies of my Linnean paper; but my letter was sent about time of

sailing of Anglo-Saxon, & were perhaps
lost. I cd remember in my letter telling
you how tight you were about fertilisation
of Cypripedium. Off to Spiers sent by
you. R. a caule alone has prevented
& has prompted me. Mitchella, alas,
does not look very healthy with all
our care. If you see & know Mr.
Sandbach please thank him particularly
for his interesting paper on Tropaeolum,
which I was very glad to read. —
To return to your Review: I was very glad to
see your Remarks in answer to Fabre's
on Hybridity: I infer you cannot explain
why there are not intermediate angles. I
have been looking at Nageli's work on
this subject, & am astonished to see the
angle is not always the same in young shoot
when leaf-buds are first distinguishable
& in full-grown branch. This shows, I think,
that there must be some potent cause
for these angles which do occur: I daresay
there is some explanation as simple as
that for the angles of the Beech-tribe. —
You allude to Saporta's work: Alph. De
Candolle sent me a copy of part of
letter from him, in which he expressed
strong belief that R. Selection would
ultimately be triumphant in France, though
not quite ignored. — I have nothing
to tell you about my own doings: I
work every day, that I can, on my big
book & am now at all causes of
sterility under domestication & cultivation.
I have got such an immense
collection of facts, that the work though
laborious & slow interests me, as I can

generally come to some sort of conclusion. There never will be a man who will read my big book: it will be a sort of encyclopedia on special ~~facts~~ cases. — I have been looking again at the imperfect flowers of Oxalis & Viola: I was entirely wrong in supposing that in Oxalis the perfect flowers received much aid for fertilization; to this view I knocked on the head. Viola, however, does receive much. I must get stick to my opinion that the imperfect flowers of Viola at least deserve to be ranked as something more than mere precocious flowers. In the carina of 2 anthers are developed: the pollen-grains are smaller — the pistil is widely different in shape: no nectar-appendages to the two fertile stamens & no spur. — Remember, if you can get them, seed of Campanula perfoliata —

May 84

I have been observing common Broom: hardly any orchid shows prettier adaptation to which an agency for its fertilisation: insects, — the upper & lower surface of the keel of Bees Thorax as gets dusted with pollen, & first the stigma rubs the upper side ~~top~~ of Thorax & afterwards is rubbed against the lower side of Thorax.

I suppose you are very busy.
& I suppose the whirlwind of
political affairs must waste
much of your time. So I
not think of writing to
me unless you have
any leisure; though a letter
from you is always a real
pleasure to me. I suppose
there are few human
beings in England who see
so few persons out of their
own family as I do. —
Good night.

Yours most sincerely

C. Darwin

Jan 26

Down Bromley Kent

[I do not know who "Hinton" is.]

My dear Gray.

I thank you for two pleasant letters; the former about Reviews in Athenaeum & many other points, & the second shorter with answers to several of my questions. In this latter you seem cruelly overwaked. Although it is one of my pleasures to write to you & a very great pleasure to receive a letter from you; I earnestly beg you never to write to me when so busy: if I did not hear for six months or twelve months, I should understand the cause. Remember what a number of valuable & most interesting letters, ~~which~~ I have received from you. So pray do not write unless you have a little leisure, which seems rare with you. — I have little

or nothing to say, for I see we are a dear
few to one except dear Hooker. But
I must just run through 10 more points
in your letters. How curious the lie
about Ohio marriages! I find it a
difficult evil in my compiling work,
not knowing what to trust. Many
thanks for references about Phyllotaxis:
I have been half-mad over it,
but am being a bull: I have
made no end of diagrams; but all
my attempts have signally failed, as
might have been expected. You will
have seen by a later letter that I
have received your Review on
Decandolle. I suppose you will have
received Bentham's address, which
has pleased me much, more than I
understand why: it will do a world
of good for our side. — But you
tell me about Phlox Drummondii being suspicious;
I have been looking at mine and remarked
P. Drummondii & that is not dimorphic.
Euphrasia, I see, is dimorphic like
Primula, i.e. hermaphrodite & female plants.
Pistillata has only two flower buds
(alas!) but we have just found
out, why it is unhealthy, viz. we force
the plants too much water. —
The seeds of Sicyos did not germinate;
& of one plant has come up of
Echinospermum: I have been looking
at the tendrils & seen with great
interest their irritability: it is a

by pretty little discovery of yours.
I am observing the plant in another
respect, namely the incessant
rotatory movement of the leading
shoots, which bring the tendrils
into contact with any body
within a circle of a foot or ~~two~~ 20
inches in ~~circumference~~ diameter. — If I can
make out anything clear about
this movement, & do not find
that it is known, I will perhaps
write a letter to you for the chance
of its being worth inserting in
Silliman or elsewhere. — Farewell. I
have written this merely to tell you
not to write, which pains that I
am one of the best of human beings,
Farewell. C. Darwin

Aug 4th  Down Bromley Kent

My dear Gray. Thanks for two letters of July 7th & 21st, full of interest to me. I am heartily glad to hear that you are so thoroughly refreshed by your short holiday; for you seemed to be almost worn out by work. It is capital news about your Legacy: I only wish it was five or ten times as much, & then you could give up all your time to next work. How good-natured you are to my little mania about stamps: those enclosed in two last letters were ineffable treasures. Many thanks for Specularia; but, good Heavens, I fancied it was a specimen to show me the flowers & I put it in warm, not boiling, water: I then discovered my mistake & hope I have not killed the seeds. Was not that a misfortune? — With your pleasing

letter came a most obliging & kind note
from Mr Brace: if you have any communication
with him, pray thank him; I wish
not write, as I had *previously* written
to thank him for his kind present of
his book. What a pleasant book his was on
Hungary. — After I had written to you
Haeckel's doctrine of sudden changes, I suspected
what you would say: what, I think, aught
to give you the sarcastic "cold chill" is the
case of Pouter, Fantail-pigeons &c: were
not these variations accidental as far as
the purpose man has put them to? Spell
said he could grapple with this, but I
suspect he found it would be most prudent
to shirk the question. In my present book
I have been comparing variation to the shapes
of stones fallen from a cliff, & natural or
artificial selection to the architect: but
I cannot at all work a metaphor like you do. —
That seems a very pretty case of the orchid with
prominence on labellum. — I have lots of
hobby-horses at present, fertility of fabric
flowers & especially of "Homotypic" seedlings,
which I suspect will throw much theoretical
light on Hybrids. — I have worked Sphenoclea
like a Trojan & have just finished 634
crosses, no slight labour: but the case seems
to me worth any labour, for I declare
I think it about to address case of sexu=
=ality even noticed. — a triple marriage
between three hermaphrodite. — I am so
glad your Hesperis, after a sickly infancy
are growing splendidly! But my present
chief hobby-horse I owe to you, viz
tendrils; their irritability is beautiful, as
beautiful in all its modifications as
anything in orchids. About the *spontaneous*

movements (independent of twining) of the tendrils & upper internodes. I am rather taken aback by your saying "is not it not well known?" I can find nothing in any book which I know: neither Hooker nor Oliver knew anything of these movements. The spontaneous movements of the tendrils is independent of the movement of the movement of the upper internodes, but both work harmoniously together in sweeping a circle for the tendrils to grasp a stick. So with all climbing plants (without tendrils) as yet examined, the upper internodes go on night & day sweeping a circle in one fixed direction. It is surprising to watch the sporgrass with shorts 18 long, beyond the supporting stick, feeling searching for something to climb up. When the short meets a stick, the motion

at that point is asserted, but in the upper part is continued,
so that the climbing of all plants set examined
is the simple result of the spontaneous circulating
movement of the upper internodes. — Pray tell me
whether anything has been published on this subject:
I hate publishing what is old; but I shall
hardly regret my work, if it is old, as it has
much amused me. — If I do publish my paper, it
will be too big to send you: for I am (as
I hope) examine my many plants. —
I am very glad you are going to review Bates' paper
& I must say I enjoy anything which tells against —
He seems to grow bigoted with increasing years: I
was saw him years ago, & was charmed with him.
Depend on it, you are unjust as to merits
of my beloved Drosera: it is a wonderful
plant, or rather a most sagacious animal.
I will stick up for Drosera to the day of my
death. Heaven knows what I shall ever
publish of my pile of experiments in it —
How profoundly interesting American news is — I declare
almost more so to us than Crimean

new. — Do not hate poor old England too much. Rather she is the mother of time children all over the world. I declare no man could have tried to wish more sincerely for the north than I have done. — My reason tells me that perhaps it will be best, — of course best if it will end slavery! but I cannot pump up enthusiasm. The boasting of your newspapers & little men, & the abuse of England, and the treatment of the free colored population, and the not freeing Maryland slaves stops all my enthusiasm. If all the States were like New England the case would be different. — I find a man cannot keep his intention. You will think me a wretched outcast. Farewell & do not hate me much. What devils the low Irish have proved themselves in New York. If you conquer the South you will have an Ireland fastened to your tail. — Good night & Farewell

C. Darwin

to guard the labellum &
confers a count of
Cogantha & the use
of the bucket-like
labellum full of water
beat everything: I
<u>suspect</u> the Bees
being [illegible] weather
flutter hence & allow
vanity fence to allow.
I have given up heavy
the newspaper cord [illegible]

80

Jul 25 Down Bromley Kent

My dear Gray

You have been so kind & good a friend to me, that I think you will like to hear a note in pencil to hear that I am better. The vomiting is not nearly so bad & on my good days, I am much stronger. My heart hardly ever troubles me [illegible] [illegible] is [illegible] — It is now six months since I have done a stroke of work; but I begin to hope that

in a few more months, I may be able to work again — I am able most days now to get to my Hot-house. I amuse myself a little by looking at climbing plants. The first job which I shall go in to draw up results of Lightning experiments on movement of climbing plants —

I have of course seen no one & except poor Dear Hooker I hear from he the like a good & true friend, though (as usual), often writes to me —

I have had one letter which has interested me pretty little a paper which will appear in Linn. Journ. by Dr. Langer of Trinidad, which shows that I am right about Cotyledon, & spot where pollen & Pistil should meet flower & I have

— Both are more among
a left trying, for —
Heaven to lift of
trembly nerves, which
I know heart —
astonishing. — I have
heard little about
Amers — from
but — can little
two age — planned
letter, which for —
month I have been

Boxing Day

Writing to answer & thank
you for — Sometime let
me see what you are
doing & what you
expect for your Country —
from your Brother
Erasmus Darwin
naturalist & physical
friend. C. Darwin

I wish Dance was not
so uncertain & fluctuating
in his writings —

many years before your
country, will I hold down to
its old position. —
I received a little time ago a
paper with good account of
your Herbarium & Library, &
a long time previously your
excellent review of Scott Riddle:
= lecice, & I forwarded it to
him in India, as it would
much please him. — I was
very glad to see in it a
new case of Dimorphism (I forget
just now the name of plant)
; I shall be grateful to hear

77

April 19 Down Bromley Kent

My dear Gray

I have not written for a
long time (& a good job
too perhaps you will think)
, but since receiving your letter
written in Jan' 19th; but I
have often thought of you
& often wished to write,
but either had other things
to do, or felt too tired.
I have nothing ~~to~~ particular
to say now, but the great
news of Richmond has

stirred me up to write. I congratulate you, & I can do this honestly, as my reason has always urged & ordered me to be a hearty good wisher for the north, though I did not do so enthusiastically, as I felt we were so heated & you. Well I suppose we shall all be proved utterly wrong who thought that you did not entirely subdue the South. One thing I have always thought that the destruction of Slavery would be well worth a dozen years war. Two days ago a very charming man, enthusiastic for the north, called on me, Mr. Langdon, & he does not believe that you will attack us & Canada. I fear it will take

Gray 77

so fearfully long, that
I do not like even
read it. If, however,
you do not <u>like</u> though
it, you will be
an unnatural parent,
for it is your child.
Believe me, My dear
Gray, Yours affectionately

Charles Darwin

of any other cases, ~~as~~ I felt
first interest in subject. I
shall be very glad to get some
seed of your dimorphic
Plantago; for I cannot
banish suspicion that they
must belong to the very
different class like that of
the common Thyme. How
could the wind, which is
a great agent of fert. with Plantago,
further a "reciprocally dimorphic" flower
like Primula. Nature says
this cannot be, & is such

cases of this on theories

I follow Agassiz & believe

"that nature never lies".

I shd very glad

to examine the 2 dried

forms of Plantago. Indeed

any dried dimorphic

plants wd be gratefully

received. You made

capital remarks, with

respect to Nectar little

imperfect flowers, on flowers

which never open —

Did my Lythrum paper interest

you? I crawl on at

rate of 2 hours per diem

with Variation under Domestication;

& I have begun correcting

proofs of my paper on

"climbing Plants". I

suppose I shall be able

to send you a copy

in 4 or 5 weeks. I

think it contains a

good deal new & some

curious facts, but it is

19

My dear Gray

Down Bromley Kent
May 28

Your kindness will make you glad to hear that I am nearly as well as I have been of late years, though a good deal weaker. I have been slowly writing a paper on Lythrum, & this has disinclined me for the exertion of writing letters. It has been so pleasant doing a little work after 8 months inaction. Speaking of Lythrum reminds me to say that your Nesseas are looking very healthy, & Mitchella moderately so. Some time ago I received Dr. Wright's letter about Orchids: if you write to him, beg him to note what attracts insects to Begonias? Do they guard a penstemon or potato? Also, but I can help, what attracts them to Melastomas? Poor Dr. Conger of Trinidad, who promised to observe, is dead. –

Whenever my Lythrum paper is printed I will of course send you a copy, & I shall like to hear whether you think it as curious a case as I do. — I have got another new sub-class of dimorphic plants. — An Irish nobleman on his deathbed declared that he could conscientiously say that he had never dampened life denied himself any pleasure; & I can conscientiously say that I have never sampled to trouble you. — So here goes: — Have you travelled south, & can you tell me, whether the trees, which Bignonia capreolata climbs, are covered with moss, or filamentous lichen or Tillandsia? I ask because its tendrils abhor a simple stick, & cut much which simply bark, but delight in work or moss. My advice in curious manner, by making little knobs at end of each point, like the Ampelopsis; then the thick sticks to bundle of fibres, these fibres grow between them & then unite, so that the tiny ~~knots~~ ends by being embedded in middle: By the way I will enclose some specimens & if you think it worth while you can put them under the simple microscope. It is remarkable how specially adapted the tendrils are; those of Eccremocarpus scaber do not like a stick, will have nothing to say to work; but give them a bundle of cotton of glass or bundle of bristles & they pledge them well. —

I have been reading with great interest your Notes paper in Bot. Gazette on imperfect self-fertile flowers. He quotes you that perfect flowers of Voandzeia are quite sterile — How is this known, for is it not a Madagascar plant? I presume you know that wild plants of Amphicarpea are generally sterile, but I d. like to have seed to ascertain whether this plant is sterile when fertilized. — What a curious analogous case is that of Leersia: I have just got plants of this genus. — I know, if you ever come across it, seed of a Commelinaceous perfoliate. — Lastly (God forgive me) can you tell me whether any of your Hollies are in state of Thymus viz some plants hermaphrodite & some Females: I have a dimorphic case which, I think, will show how this state of Thymus & arises. — Do you see the Reader: it is the best newspaper periodical for science ever published in England: there was lately a capital article in it, by Wallace. He has, also, published lately in the

Gray 79

Anthropological Review a short, but most suggestive article on the natural selection of man. When inclined, do write & tell me a little about yourself & what you are doing. Is the museum for your Herbarium settled? I hope sincerely that my former fellow-sufferer Mr. Gray is quite well again. What utter misery the stomach causes! — Excuse for putting this in I am much, sir, I gave up for months reading to newspapers, as I found it more fatiguing than books. I have ~~to~~ heard during late 9 months an astounding number of bad sermons —

What dreadful carnage you have just recently suffered. — What will the end be? Will slavery perish, if no the cost is not too dear?

Farewell my dear & good friend. You will see that I have regained my ten-horse-power-strength: — farewell — you must be very

C. Darwin

I send a Photograph of myself with my Beard. Do I not look venerable?

it seems to me a most
remarkable production
though written rather
obscurely in parts, but
worth the labour of
studying. — I have just
bethought me that for the
chance of your noticing
it in to Journal, I will
point out the new & very
remarkable fact. — I
have paid the poor fellow
passage out to India,
where I hope he will
succeed, as he is a

89v

Sept 13th

Down.
Bromley.
Kent. S.E.

My dear Gray

I have taken a long time
to thank you for your
pleasant & most friendly
note of July 11th; I am not
ungrateful, but I have lost
strength (though still gaining
some & now at least
living down stairs) then
formerly & often my two hours work
seemed to be quite idle.
I have little to say; for
my work has been absorbed
with climbing plants, now

finished & tomorrow I begin again, after 13 months interruption on "Variation under domestication"! —

I received lately a Review on H. Spencer with the address in your hand-writing: I like much all the latter part; but surely you did not write it! If you did you had muddled your brains (in the first part) by reading metaphysics & all elasticity had gone out of your style. —

Oh, if you did write it, I hate I not have some nice little stabs with your sharp dagger! I also before received a review on Dana, I believe in your hand-writing: & this interested me considerably & I thought it was perhaps by you. —

I write now chiefly to say that I send by this post a paper which has interested me greatly by a gardener John Scott;

Mr Lincoln & other
men, with the manners,
almost of a gentleman —
We are profoundly interested
by your politics; & do not
in the least know whether
the "old Bloody Times"
is to be revived that
there will be peace &
that the middle States
will join with the South
on Slavery & eject
the northern States. —

In the latter case, I
hope you will marry
Canada, & divorce
England & make
a grand country,
counterbalancing the
detestable South —

Yours affectionately

Charles Darwin.

Scott's paper

p. 106–108. Red cowslip of variation has become a non-dimorphic, & with this change of structure has become almost more productive of seed than even the heterostylic union of the common cowslip. – p. 91–92 similar case with Auricula – On other hand a non-dimorphic var. of P. farinosa (p. 115) is less fertile. These changes & variations in the generative system seem to me very remarkable.]

[But far more remarkable is the fact that the Red Cowslip (p. 106–8) is very sterile when fertilising, or fertilised by the common cowslip. Here we have a new "physiological" species. – Analogous facts given

(p. 98) on the crossing of Red & White Primroses with common Primroses. —

It is very curious that the two forms of the same species (p. 93, ~~94~~ 94, 95, + p. 117) hybridize ~~unite~~ with extremely different degrees of facility with distinct species. —

He shows (p. 94) that sometimes a cross with a quite distinct species yields more seed than a ~~homomorphic~~ homomorphic union with one pollen.

He shows (p. 111) that of the two homomorphic unions (spith with each dimorphic species) that the short-styled, (as I stated) is the most sterile, & that by fertilization is perhaps true. This is good Summary of Paper. —

new lot of work for me & I have been pleased to find that a capital guide for discussion, a fair criterion of the change of species is. —

We always like to hear your opinion on public news: my wife is indignation has changed the Times for the Daily News, which I find rather dull, but it does not much concern me, for I read but little & live a sickly frolicsome novel which one reads about to me & my dear workwoman.

Farewell my good friend, do not write one word as you are a slave to your work. Farewell. — C. Darwin

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Down.
Bromley.
Kent. S.E.

Oct. 29th

My dear Gray

Thanks for your interesting little note of Oct. 3rd. — you allude to a question on which I have received & am sorry for. — I have little or nothing to say & it is no wonder, as I live so uniform a life. — It is a horrid shame, busy & overwhelmed as you are, but I write chiefly to ask you to get for my Ornithologist or Odologist answers to enclosed questions, as far as may be possible. — I know that there is some good

than at Cambridge or Boston, the
name I have at present
forgotten. —

Do read in last Nat. Hist.
Review Huxley on Kölliker
& Flourens: you, yourself
could not have done it
better. — I had a letter
a little time ago from
a good believer in change
of species, viz B. Walsh of
Illinois. — There are good
philosophical remarks in his
paper, & for an odd case,
philosophy is rarely found
in entomological works

I am able now to walk on my good days
for about 2 hours. — I think Phosphate of
iron, which I have in often used with
you, has done me good. Lady Gell
was giving me a wonderful account of
the benefit a dyspeptic lady had
received from a Philadelphia medicine,
which is imported into England & is
called "Syrup of Phosphates". Did you
ever hear of it? I am tempted to
try it, if I knew what it was
composed. — I am plodding on with
little success on "Laws of Variation"; &
have succeeded only in making a
disjointed skeleton on which to hang
a multitude of queer facts. But I
have not been able to resist doing
a little more at your God-child
my Climbing paper or rather in
this little Book, which by June I
will have copied out, when I shall
work it up. This has been

Aug. 15

Down.
Bromley.
Kent. S.E.

My dear Gray

I was much pleased to get your letter of July 24th — how that I can do nothing, I maunder over old subjects & your appreciation of my climbing paper gives me very great satisfaction. I made my observations when I could do nothing else & much enjoyed it, but always doubted whether they were worth publishing —

Mr Wallace has enjoyed his American visit greatly, & has received to much wonderful amount of American hospitality. — This is a longer note than I have written for many weeks, so farewell. I am trying a starving system of cure; eating very little of anything, & that almost exclusively bread & meat.

Your affectionately
Ch. Darwin

dear Asa Gray

I demur to it not being necessary to explain in detail about to spikes in caught tendrils running in opposite directions; for the fact for a long time confounded me & I have found it difficult enough to explain to 2 or 3 persons. One botanist has published that he could detect a difference of structure in the tendrils at the point of unusual of to spike! Very many thanks for Specularia seed. We continue to be deeply interested in American affairs; indeed I can for nothing else in the Times. How egregiously wrong we English were in thinking that you could not hold the South after conquering it. How well I remember thinking the Slavery and Slavrick for centuries in your Southern States! My women read much aloud to me, & I have lately heard three Books, worth your attention — Lubbock Prehistoric Man — Tylor early History of Civilization, which is admirable; & Lecky's Rationalism, which

with its own pollen. I have also ascertained that plants raised from Dimorphic species fertilized by their own pollen, are themselves generally sterile, & are often dwarfs, so that they offer the closest analogy with Hybrids; the first cross & the product both being more or less sterile; this seems to me a very curious fact.

I do not know when I shall be able to publish any of these results, for I have resolved, whenever able to do any thing, to publish my next book.

I have not heard very lately from Hooker, who I believe returns tomorrow to Kew. His illness has been very serious. To me the loss of his correspondence has been very great.

My wife has read aloud to me Stephens' 2 books on Central America. What a remarkably pleasant writer he is! & how singularly deficient in the spirit of a naturalist. Who is he & is he still alive

we liked his books

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Extremely; & will cost it thankfully for the sake of your kind feeling & you are enough to stimulate a dead man to work. from effect

Down.
Bromley.
Kent. S.E.

Oct. 19th

My dear Gray

I received yesterday your article on climbers & it has pleased me in an extraordinary & even silly manner. You pay me a superb compliment, & as I have just said to my wife I think my friends must perceive that I like praise, they fill me such hearty doses.

I always admire your skill in reviews or abstracts, & you have done this article

excellently & given the whole
sense of my paper. — I daresay
you are right about the
climbing roses; I never thought
about spring shoots behaving
differently from others. I have
had a letter from a good
gardener in S. Brazil, F.
Müller, who has been tried
up to observe climbers &
gives me some curious cases
of Branch-climbers, in
which branches are converted
into tendrils, & then
continue to grow & throw
out leaves & new branches
& then lose their tendril
character.

I am certainly better & have
not vomited for above 5 weeks.
This is certainly due to not
having eaten anything but
toast & meat for the last 2 months.
But I cannot recover mental
strength & I do no regular work.
I have had some flowers crossed
for me this summer & have lately
been counting seeds.

You might like to hear that
Mitchella behaves exactly like the
Cowslip. Did I ever tell you that a
year or two ago I ascertained that
Pulmonaria offers a curious case.
The long-styled form being absolutely
sterile with its own pollen, whilst
the short-styled is almost perfectly

April. 16th

Down.
Bromley.
Kent. S.E.

My dear Gray

I have been a scandalously ungrateful, & idle dog for not having thanked you very long ago for the second article on climbing plants, which pleased me greatly. But, as I have before said, now that I work a little, I seldom feel inclined to write to anyone, but this evening the spirit has moved me to do so, though I have little to say, & my dear Commencing

chanted with to eject pollen from a distinct flower can do the work of fertilisation.

I was well at work on my new Book, when in beginning of March, Murray required new edit. of Origin, & I have been correcting & adding matter of some importance even since. It almost broke my heart to give up so much time, but I have been comforted by finding that it will likewise serve for a new German edition, which is wanted.

is poorly with influenza, which has lingered on us for a week. — Horkel paid us a visit of a day about a fortnight ago & I was delighted to see him looking well & in good spirits: he hopes before long not to be so terribly overworked & is going soon to write a paper on the St. Helena Flora from Burchell's collections. I have lately had a letter from Fritz Müller in S. Brazil, full of curious observations. One case, which he is going to publish in Germany, is of a Rubiaceous plant with very long tubular corolla & with stigma in the middle: when an insect or any object touches the filament of the stamens, they suddenly & violently bend & eject the pollen, which had been previously shed & collected into a ball between the anthers, against the intruding object. But the curious point is that this same movement closes the tube of the corolla, so that the insect cannot then fertilize the flower; but in about 8 hours the tube opens & then an insect

All this has made me regret (2
<u>extremely</u> that the American
edition was stereotyped; for
the book is now evidently
unfitted for what it was
in to 2d. edition, which is.
to be reprinted in
America. — I suppose nothing
can be done: the corrections
are far too numerous & minute for attention in
stereotype. I presume the
sale has stopped; & even if it had not quite stopped,
it would, I fear, be
useless to ask Publishers,

Gray 96

Either the same or any other, to bring out an amended edition. — I will send you a copy whenever it is published, for the chance of you liking to have one. —

I will first ask on bare chance, whether you have any <u>new</u> facts on the <u>direct</u> influence of pollen on the first growth of the mother-plant; for this subject has come to interest

me greatly: also whether you know of any case of but with blended character produced at junction of stock & graft: I have been reading a paper by Carpenter on the subject.

I hope all the Fenian row in your country, will not be the cause of more trouble & hatred between our two countries. It seems blowing over at present; as

Gray 96

I hope on your tombs
about your President &
to South. I dare say
I can hardly yet realize
the great, magnificent
fact that Slavery is
at end in your
country. Farewell
my good & kind
friend —

Your most sincere
C. Darwin

I work daily now between
2 & 3 hours! & walk
3 or 4 miles daily!! yet never so safe

would be incapable.

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Down.
Bromley.
Kent. S.E.

Aug 4th 1859.

My dear Gray

I am going to write only a line to thank you most cordially for all the great trouble which you have taken about the new edit. of the Origin. From what you say it is evidently hopeless, & I am sorry for it for my own sake & for all your labour in vain. I will let Murray hear your suggestion of sending copies to the U. States. Although the book is complete & bound, Murray for trade reasons will not sell

Cambridge 27th Aug. 66

Dear Mr Field

Perhaps you will drop me a line, which I can forward to Mr. Darwin, to encourage him in his disposition to send an advance sheet of his new book.

Truly Yours

Asa Gray

it till November; but he promised to send a copy to you.

I hope to begin printing my new book towards the close of the year & I will send sheets as printed in hopes that you will have the great kindness to agree for an American edition. What a misfortune for scientific works is your american plan of stereotyping.

With cordial thanks believe me My dear Gray

Yours most sincerely

Ch. Darwin

expence if metal copies were procured of these, & to do this Messrs Ticknor wd have to make up their minds soon & enter into some arrangement with Murray, as I think I cd not ask Murray, who publishes at his own risk, to give copies; though as far as I am con:=cerned I wd willingly do so. I feel pretty sure that I shall not even begin to print till the beginning of next year.

Many thanks for the specimens of the Rhamnus; my son & self have both looked at the pollen of both forms but alas! can make out no difference. The difference seems confined to the pistil & to the peduncles. We cannot even con:=jecture whether this species is recipro:=cally dimorphic like Primula or is merely tending to become dioecious. This is a great disappointment to us &





Down.
Bromley.
Kent. S.E.

Sep 10

My dear Gray

Perhaps you will remember when one of my boys was terribly ill you sent him some stamps which he looked at, & then after a long silence there came out the words "He is awfully kind." He never said a truer thing. In the 1st place your note about Agassiz has interested & amused me much; for the day before I had been reading the Atlantic Monthly & the copy of a letter from Mme Agassiz to Lyell & one from him

all about the Amazonian glacier. We were both lost in astonishment at the nonsense which ~~he~~ Agassiz writes & I cd not resist sending to Lyell a copy of part of your note, for his pre-determined wish partly explains what he ~~thinks~~ fancies he observed. The evidence advanced by him is so weak that I do not think it wd be admitted for the former existence of glaciers even in a temperate region.

With respect to the Origin, you speak of reading the sheets, but Murray promised me to send you a bound copy. After all ~~of~~ as there is no chance of a new edition perhaps it wd be as well to let the Appletons have the sheets if they wd make any use of them; though on the other hand it is hardly worth while taking trouble about giving a few of the additions.

With respect to my next book "on domestic animals" I am in perplexity, though most grateful to you for the capital bargain which you have made with Mess^rs Ticknor. My perplexity is this that I really have no idea whether it will be in the least degree popular; I am sure some chapters are curious, but then many others enter into far too minute details for the general reader: hence I do not quite like Mess^rs Ticknor to agree to publish until they have seen some of the sheets. And here comes the difficulty; there are about 42 wood-cuts & it wd save great

the nature of the two forms cd only be made out by experiment or by observing their seed-production in their natural state.
I suppose this species cd not be purchased in your nursery gardens.

In my last letter I asked you whether you knew of any striking cases of endemic or naturalized plants which never flowered or which never seeded; if at no time I get an answer I shall understand that you know of no cases like the Acorus or horse-radish in Europe.

The only point which I have made out this summer which

cd possibly interest you is that the common Oxlip found every where more or less commonly in England, is certainly a hybrid between the primrose & cowslip; whilst the P. elatior found only in the Eastern Counties (Jacq.) is a perfectly distinct & good species; ~~but~~ hardly distinguishable from the common oxlip except by the length of the seed-capsule relatively to the calyx. This seems to me rather a horrid fact for all systematic botanists.

I have just begun a large course of experiments on the germination of the seed & on the growth of the young plants when raised from a pistil fertilized by pollen from the same flower, & from pollen from a distinct plant of the same or of some other variety. I have not made sufficient experiments to judge certainly, but in some cases

the difference in the growth of the young plants is highly remarkable. I have taken every kind of precaution in getting seed from the same plant, in germinating the seed on my own chimney-piece, in planting the seedlings in the same flower pot, & under this similar treatment I have seen the young seedlings from the crossed seed exactly twice as tall as the seedlings from the self-fertilized seed; but both being germinated on same day. If I can establish this fact (but perhaps it will all go to the dogs) in some 50 cases, with plants of different orders, I think it will be very important, for then we shall positively know why the structure of every flower permits, or favours, or necessitates an occasional cross with

a distinct individual. But all this is rather cooking my hare before I have caught it. But somehow it is a great pleasure to me to tell you what I am about.

Believe me my dear Gray
ever yours most truly
& with cordial thanks

C. Darwin

the very complicated manner in regard to their fertilization by Lythrum. — I am going on with my trials of the genus of plants raised from self-fertilized & crossed seeds, & begin now to suspect that the wonderful difference in ~~them~~ genus & constitutional vigour occasioned by with crossing plants which have been raised for several generations in England, but which are not properly visited by insects & so have been rarely crossed. —

I have just heard of a case which has interested me hugely, & which I am



April 15

Down.
Bromley.
Kent. S.E.

My dear Gray

How good you have been to take so much trouble about the Experiment queries. — I wish I had thought earlier of having them printed, for in that case I might have sent a dozen to each of my ~~co~~ few correspondents, as it is I can think of no one to send them to, so do not want any more.

By the way I have just thought of Thwaites in Ceylon & will send him a couple.

I have been lately getting up & looking over my old notes on Expression, & fear that I shall not make as much of my hobby-horse, as I thought I could: nevertheless it seems to me a curious subject, which has been strangely neglected. I have seen no one for months (but Hooker I expect to say with us here next Saturday) & have no news. — I am plodding on heavily correcting, & trying to make an atrocious bad style a little better, my book "on the Variation of Animals & Plants under Domestication": I could offer to send you clean sheets, but I do not think you would care to receive them. There is not much about plants, & what there is is almost all new compilation: it will be a fearfully big book in two vols. & I shall be to next Feb or March merely correcting the proofs: it is enough to make one curse one's fate in being an author. — I manage to get a little amusement by some of my experiments. — I have proved that the trimorphic species of Oxalis behave in exactly

inclined to believe in time;
namely the of cutting the
tubers of different closely
potatoes except to eye, &
joining them, you can make
a hybrid or mongrel.

I am repeating this experiment
on a large scale, for it
seems to me, if true, a
wonderful physiological fact.
Here is a long prose about
my own doings.

Farewell with my thanks

Yours most sincerely

Ch Darwin

This verifies what I said in the Origin, that many new methods of transport wd be discovered; for locusts are often blown many 100 miles out to sea.

The rest of the sheets, which have all been corrected, will be printed off by the middle of Novr. & shall be sent to you in 2 or 3 packets. Do not forget to let me have hereafter a copy of the Nation.

My dear Gray
yours most sincerely

Ch. Darwin

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Down.
Bromley.
Kent. S.E.
Oct 16

My dear Gray

I send by this post clean sheets of Vol. 1. up to p. 336, & there are only 411 pages in this vol. I am very glad to hear that you are going to review my book; but if the Nation is a newspaper I wish it were at the bottom of the sea, for I fear that you will thus be stopped reviewing me in a scientific journal. The first Vol. is all details, & you will not be

able to read it; & you must remember that the Chapters on plants are written for naturalists who are not botanists. The last Chap. in Vol. 1 is, however, I think a curious compilation of facts; it is on bud-variation. In Vol. 2 some of the Chaps are more interesting; & I shall be very curious to hear your verdict on the Chap. on close inter-breeding. The Chap. on what I call Pangenesis will be called a mad dream, & I shall be pretty well satisfied if you think it a dream worth publishing; but at the bottom of my own mind I think it contains a great truth. I finish my book with a semi-theological paragraph, in which I quote & differ from you; what you will think of it I know not.

Many thanks for a 2nd note recd some time ago on Dionaea.

I have done nothing worth mentioning this summer, as all my time has been consumed in correcting horrid proof sheets. I may mention one little fact which may possibly interest you. A man in Natal sent me a little packet by post of the dung of locusts with the statement that it was believed that locusts brought new plants to the districts which they visited. Six Grapes, belonging to at least two species have germinated out of the dung, & the seeds were fairly enclosed in the little pellets, as I ascertained by dissection.

Down.
Bromley.
Kent. S.E.

May 8

My dear Gray

I have been a most ungrateful & unpraiseworthy man not to have written to you an immense time ago to thank you heartily for the "Nation" & for all you must think did in regard to the American Edit. — But I have been of late overwhelmed with letters, that I was forced to answer & so put off writing to you. — This morning I received the American Edit (which looks capital), with your nice Preface for which hearty thanks. I hope to heaven this the Book will succeed with enough to prevent you repenting of your aid. This

whatever holds good in the formation of a fancy pigeon holds good in the formation of a natural species of pigeon. I cannot see that this is false. If the right variations occurred & the others reduced to be superfluous — selection

A Reviewer in an Edinburgh paper, who treats me with profound contempt, says on this subject that Prof. Asa Gray could with the greatest ease smash me into little pieces. Believe me, my dear Gray

Your ungrateful but sincere friend

Charles Darwin.

arrived here just to finishing
Mother to my conscience,
which will endure its wrongs
no longer. I received, also,
this morning a very pleasant
& friendly letter from Mr Darwin
& shall write in a
few days. — He has sent me
a bundle of the agricultural newspapers,
which seems to contain an
astounding miscellany of facts
& notions.

Your article in the Nations seems
to me very good & you give
an excellent idea of Pangenesis,
— an infant cherished by few
as yet, except his tender parent,
but which will ~~yet~~ live a
long life. There is paternal
presumption for you! You
give a good slap at
my concluding metaphor: undoubtedly
I ought to have brought in
& contrasted natural & artificial
selection; but it seemed to
~~me~~ strike me that natural selection
depended on contingencies even
more complex than those which
must have determined the shape
of each fragment at the base
of my precipice. — What I
wanted to show was that
in reference to foreordainment

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Aug. 15 - 1858 Freshwater
Isle of Wight

My dear Gray

Hooker left us yesterday for his Presidency of B. Ayres. I tell you (for he forgot to do so before) the wonderful & good & kind Gray news that you & Asa are coming to Europe for a year. I must write now to say how very glad I am. You must pay us a visit after

down & see me. Solitary &
my quiet life. We
have been staying here for
a month & go home next
week. I was bed for a
month before starting & could
do nothing & to my great
vexation this outing has done
hardly anything for me & I
I must do all better I can
resume my small medicine
of work. – I hear that
you are coming here to begin
a quiet work & not for
sleep, which I should think

would be best for you.

I wish you could have attended to B. Apr. under the feeling of our good & dear friend; it has been a great disappointment to me that I had not time enough for the undertaking.

Farewell then. I have the very great pleasure of thanking you & to hand & of seeing, as I hope, Gray. Farewell.

Your sincere friend

Ch. Darwin

I have little to say about my own doings. My regular work has been much interrupted by preparing a new Edit. of the everlasting Orig. Species, which consumed 8 weeks & by preparing a French Edit. of my Orchis book. In these notes I give references to all papers on subject, with very brief abstracts; so that I have had to quote you inopportunely. I think I shall also publish these notes in English. By the way I tried to get Longs Judd & Co to publish

June 1st

1867

Beckenham — ~~Crawley~~ Down. Kent. S.E.

My dear Gray

I was uncommonly glad to get your letter of May 8th & it was extremely good in Mrs. Gray to write to me so nice a note. Very many thanks to her & to you for the answers about Erysipelas. I will know how extremely difficult it is to observe. One of her answers about the "grief murder" in a negro is of especial value, as I have failed on

the head will all the more
distinct sense of man, & began
even to doubt whether it could
be general. The case of the
shuffling to shudders will also
be useful, & that about
the head not being shaken beto=
=rally for negation, is very
disagreeable & inspiring.
Very many thanks for all
your kind expressions about
my accident: my head felt
& partly rolled over me. Pagel
thought I ought rather then
months to recover, but I was
sure of well in 3 weeks. I have
however, had more pain lately
& not been very brisk. My
good & dear wife, in consequence,
is going to take me, unless
others, on to 10th to a house,
which we have hired in N.
Wales for 6 or 7 weeks.
We also heartily wish you
could have given a better
account of Mrs. Grey's health;
it is very disheartening that
the voyage shd have driven
away so much of the good
effects of your great trip
up to Wales. —

the new Edit. of the Origin; but to
Appleton were too strong for
me. My very courtesy prevents
them: fear of their pockets, I
persuade, is more to the truth. What
an evil this slovenly pirating sys-
= tem is. — I might to
tell you that they sent me 50 £
for the "Variation under Domestication",
which I think very handsome,
which I owe to you.
I have not heard from Hooker, but
I see in Gard. Chron. that he is
elected a President for next
Congress. He was received with
great applause. — We have
very hardly a cold, for a

long time except to Huxleys, & two
detachments of Hooters. I then
verified a good generalisation,
which I once propounded to
you, that all persons from
the U. States are perfectly
charming. Farewell, my dear
Gray, I often think with
pleasure of your visit here.
Give from all of us our
kindest remembrances to Mrs.
Gray & believe me
Ever yours very sincerely

Ch. Darwin

P.S. You know how my odd point I was about:
look whether the beard of Germans, when differing
in tints from Hair of head, are of a lighter
or redder tint? Does the beard differ in
tints?

March 15

Beckenham ~~Bromley~~ Down. Kent. S.E.

My dear Gray

You & Mrs. Gray have been very good, & I thank you for your letters & enclosures. — If Mrs Gray can learn anything authentic about the nursing of her dog, I am sure she will inform me. — Two days ago my wife received a postcard to the effect from Miss Mitford that Life, minutely describing a dog, which had been nursed

quite as good a wriggler as I am. — He is carefully being & we did not see him, which was a great loss. I suppose you will have seen an notice of Rowan Tyl. off Mauritius: Sir Richard himself would not be able to explain how to plant & cultivate yet distributed on these 2 islands. — I have just received from Jermyn it to Greenhouse; let us see if it will do not. Your letter shall be read & discussed. —

Yours sincerely

Chas Darwin

of a cat & which tickled its
paws! But as this is a
second-hand account, it
will not do to quote;
& the description of the
cat-like habits of this
dog was too much even
for my capacious gullet.
Whenever you have any com-
=munication with Lepsius,
pray give him my cordial
thanks for his kind message
& for his information, which
I much feared would
prove negative. I
wish with all my heart that
I could feel that I deserved
what Alex. & Lady Mary so
We have just returned from
a week in London, where we
went for a rest to me, as
I was pretty well worn out.
I find the management of
my interesting but very difficult;
& the difficulties of the Central
Sense has caused me
much labour. But as
Horace facetiously says of
me, "Do you wish wriggle
out of anything". But as I
tell him, he has become

matter. Now in yr Drosera the filaments are not sensitive to a rough touch or to any prepare from non-nitrogenous matter, but are sensitive to solid or fluid nitrogenous matter. Is it not curious that there sh^d be such diversified sensitiveness in allied plants?

I rec^d a very obliging letter from Mr Morgan, but did not see him, as I think he said he was going to start at once for the continent. I am sorry to hear the rather a poor account of Mrs Gray to whom my wife & I both beg to be very kindly remembered. My dear Gray

Yours affectionately
Ch. Darwin

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Beckenham ~~Bromley~~ Down. Kent. S.E.
Dec 7. 1870

My dear Gray

I have been very glad to receive yr letter this m^g — I have for some time been wishing to write to you, but have been half worked to death in correcting my uncouth English for my new book. I have been glad to hear of yr cases ~~of~~ appearing like incipient dimorphism. I believe that they are due to mere variability, & have no significance. I found a good instance in Solan

prostrata, & experimented on it, but the forms ~~pres~~ did not differ in fertility. So it was with Amsinck-ia of which you told me. I have long thought that such variations afforded the basis for the deve[illegible]-ment of dimorphism. I was not aware of such cases in Phlox, but have often admired the arrangement of the anthers, causing them to be all raked by an inserted proboscis. I am glad also to hear of your curious case of variability in ovules &c.

I said that I had been wishing to write to you, & this was about yr Drosera, which after many fluctuations between life & death, at last made a shoot, which I cd observe. The case is rather interesting; but I must first remind you that the filament of Dionæa is not sensitive to very light prolonged pressure, or to nitrogenous matter, but is exquisitely sensitive to the slightest touch. In our Droseræ the filaments are not sensitive to a slight touch, but are sensitive to prolonged pressure from the ~~slightest~~ smallest object of <u>any nature</u>; they are also sensitive to solid or fluid [illegible]

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Feb. 5th

Beckenham Down. ~~Bromley~~. Kent. S.E.

My dear Gray

It is a shame to trouble so hard-worked a man; but if you can, (which I doubt) will you send the enclosed to any one who has charge of Laura Bridgeman, & beg for an answer, with any details, to the one query viz. No 13. — I'd say and like to know how this is. I have been reading Lieber's paper, & I see he states that L. B. when astonished,

raise both hands with fingers
apart, & open palms
directed towards the person
causing astonishment.
This has led me to
enquire about the gesture
of impatience, i.e.
shrugging the shoulders with
its accessories.
I have finished & book a
the "Descent of man so", &
its publication is delayed by
& to Friday: when published, I
will send you a copy, but I
do not know that you will
care about it. Parts, at
on to moral sense, will
I dare say aggravate you; &
if I hear from you, I shall
probably receive a few
stabs from your pointed
stiletto of a pen. — Pray
give my kindest remembrances
to Mrs Gray: I am not
at work on my species as
hypothesis, & I have found
the few notes of great use.

Yours most truly
Ch. Darwin

101.

July 15th

Down,
Beckenham, Kent.

My dear Gray.

Two of my sons, George & Francis, intend start on the 29th for New York for a ten-week tour in the States. They will come to Boston & Cambridge towards latter part of Oct, & I am sure that you will allow them to call on you.

But I want to beg a great favour of you. Do you chance to know any pleasant persons to whom you could give

them letters of introduction
in some few of selected
places, which they intend
to stop at. My own
already a good many
letters of introduction, to do
not trouble yourself (& I
know how busy you are)
unless you happen to know
with any pleasant &
great name persons. —

I received some time ago a
delightful notice of my
Descent of Man, which
must have been written
by you.

Believe me
My dear Gray
Your most sincerely
C. Darwin

If you can send any
letters please enclose them
pretty soon to address
on enclosed paper.

My wife sends & I join in my kind

Remembrances to Mrs. Gray.

77

Down
Beckenham
Kent
Jan. 15 - '72

My dear Gray,

I have taken up an old subject which formerly interested me, namely the amount of earth brought to the surface by worms. I want to know whether you have in the States the little vermiform piles of earth which are so common on our lawns fields woods & waste lands. - Are they as numerous with you as they are with us?

I should have assumed that this would naturally be the case had it not occurred to me that the severe winter might make all the difference. – A very few lines in answer would suffice. – It just occurs to me that if you had any correspondent in the Northern parts of Canada you might send this letter to him and thus get me some additional information on the subject. – Overworked as you are I hope you will forgive me for troubling you.

Many thanks for Agassiz's letter which from the handwriting I infer you sent me. Pray give our very kind remembrances to Mrs Gray and

Believe me
Yours very sincerely
Ch. Darwin

which I am glad to
perceive, though I am surprised
it was thought worth
painting & engraving.
You cousin to Beaux
am coming here to day
after tomorrow to dine & sleep.
By the way, you will remember the
you wrote about Dr Packard.
As soon as I saw in Nature
that he was in London, I
wrote to him to call of the
Editor, but heard in consequence
that Dr. P. had started for
Paris, & whether he was
received by invitation to Brown
I know not. My dear Gray
you ever very sincerely
Ch. Darwin

107

July 8

Down,
Beckenham, Kent.

My dear Gray

Very many thanks for your
book "How Plants behave":
it is a capital idea,
capitally executed. — It has
in many ways delighted
me, & I am even more
delighted to hear that you
think of publishing in
extenso on subject. Can you
suppose your idea that
tendrils become irritable
after clasping an object,

from the stimulus from
contact running down
them, in as much
as they become thirsty
when they have charged
nothing? — I am now
correcting proofs of my
Mexican book on Explorations;
& when that is done, I
hope (health permitting &
of course not too late)
to begin on Dorsetshire.
I am thinking of republishing
also my quasi-botanical papers
with 2 or 3 new ones in a
volume. — I hope it may
be in time for you. —
I am astonished at
Mr. Gray's spirit &
audacity in going
all the way to California;
though I to have been there
in not much after
to Nile. — It makes
my blood run cold to
think of such expeditions.
I forgot to thank for
engraving of the ape-man,

can perhaps be left, as the
a stimulus to the other half
cannot be uncut. It is quite
like Dividing the Wind nerve
of a Frog: no stimulus can be
sent from the Brain or external
part of spine to the hind legs;
but if the cells are stimulated,
the more of reflex actions.
I find my old results about
the inhibitory sensitiveness of the
nervous system (!?) of Drosera
to various stimulants fully
confirmed & extended. —
I want to beg you to make
for me next spring two Sundews

100
Oct. 22

Down,
Beckenham, Kent.

1873

My dear Gray
It was very good of you to
write me your long letter of
Oct. 6th, telling me of your &
Mrs Gray's wonderful doings,
about which I was very glad
to hear. — Alas I never
received your Darwin address, but
I have seen a short extract from
it on Sequoia. — One word
more about Tendrils: I could
gladly accept your view to account for the
spirited winding of a tendril, which

has clasped nothing, had it
not been for the fact of
the same tendril, when it
has clasped an object,
contracting in opposite direction,
& in equally close spiral contractions. —
The concave side of the lower
part, in the latter case,
can hardly have contracted.
I think I have explained the
proximate cause of the reversed
spiral curvature, but I cannot
understand the more remote cause.

I have worked pretty hard for 4 or 5
weeks on Drosera & then broke
down; so that we took a house
near Sevenoaks for 3 weeks (when
I now am) to get complete
rest. — I have very little
power of working now & must
put off the rest of work
on Drosera till next Spring,
as my plants are dying.
It is an endless subject, &
I must cut it short, & for
this reason think not so much
on ~~Drosera~~ Dionaea. — The
point which has interested
me most is tracing the nerves !!!
which pass to vascular bundles.
By a prick with a sharp
lancet at a certain point, I

Gray 100

Down,
Beckenham, Kent.

Dr D. filiformis, when growing vigorously & on a warm day. — I had the new specimen to experiment upon; but am afraid of trying to my results on 2 points.

I write on next page the 2 experiments, (not difficult), & will you please keep the paper & try them for me next sunny spring. — You are most kind

C. Darwin

My wife sends kindest remembrances to you both. —

Drosera Filiformis

(1) Put small atom of crushed fly on leaf near apex, & observe whether the solid leaf itself, after 24° or so, curls over the fly. This has nothing to do with the move-ment of the glands or hairs.

(2) Rub roughly with point of fine needle half a dozen times a few glands & observe whether they become inflected after a few minutes, or more probably after a few hours.

My results were negative on both these points, but I think it likely that they were so owing to want of vigour in the plants

Oct. 22 /72/ Ch. Darwin

cultivated plants.

Owing to your kindness I have received a letter from Mrs Treat of Vineland, saying that she wd observe D. filiformis next summer. You will see by this letter that I am obeying your orders & working on Drosera &c.

Yours most sincerely

C. R. Darwin

102

Down,
Beckenham, Kent.

Jan 8. 73

My dear Gray,

You sent me a letter dated Wilmington July 9. '67 about Dionæa. It has no signature but you refer to it as written by Mr Canlay or Canbay or Cawley. Will you be so kind as to write the name for me distinctly, for some people are so foolish as to say that your handwriting, like mine, is not very legible. The letter has interested me much in some respects & the gentleman seems very kind & willing to oblige.

Does he reside at Wilmington now? And if so would you ask him to observe one point for me – namely whether the Dionæa catches large or small insects. It wd be advisable to gather a dozen or score of leaves which are <u>quite closed shut</u>, bring them home, & open them. As "large" & "small" are such vague terms it wd be very advisable, if not causing too much trouble, to measure the breadth of the broadest part & length (from end of head to end of abdomen) of an average sized captured insect; & then state how many exceeded or were less than this mean. But it sh. be particularly stated whether any of the captured insects were quite minute. Does your friend still abide by his conclusion that a leaf after catching an insect never acts again? This agrees with my small experience with

106

Frank 11

Down,
Beckenham, Kent.

My dear Gray

It is really astonishing that Agassiz shd. use such an argument as that of the Times. I have sent the memorandum to Nature; but I believe that they receive so many articles that they can find room for only a small portion of them.

I wished Cat. Newman had done Drosera, but could not finish it. I get fresh plants, & consequently

took up to effect of capping
& self-fertilising plants & can
get no intimation this Drosera
must go to the dogs this.
I finish with this & get
it published; but then I
will return my beloved
Drosera & I heartily
apologise for being such
the prime idiot thus
even for a moment to
the dogs.

I hope you are well

& heartily I do not
doubt am working like
a Trojan. My wife
takes me on Friday
as an object
prisoner to London for a
month & I do hate
stopping work. —
I am just like the
retired Tallow-chandler

Your friend

Ch. Darwin

My letter for to Dickinson & Dup. which I
received before & read with the greatest
interest.

Jan 3d

Down,
Beckenham, Kent.

My dear Gray

I was rejoiced to see your handwriting again in your note of the 4th, of which more anon. I was astonished to see announced about a week ago that you were going to write in Nature an article on me, & this morning I received an advance copy. It is the greatest thing ever written about me, especially as coming from a man like yourself. It has deeply pleased me, particularly some of your like

charge and opinions to, but not identical with Lepine; for I have been making a long series of experiments. No human being with between what I take particular about the medicine of the bones of phosphate of ammonia which act. Yesterday I found out that Insectivora digest & then absorb animal matter; I know this this holds good for albumen, gelatine & insects, but I am now in the middle of my observations.

I began reading the Independent quite gravely, & then

My dear Gray

I was rejoiced to see your handwriting again in your note of the 4th, of which more anon. I was astonished to see announced about a week ago that you were going to write in Nature an article on me, & this morning I received an advance copy. It is the greatest thing ever written about me, especially as coming from a man like yourself. It has deeply pleased me, particularly some of your like

remarks. It is a wonderful thing to me to like to see my name coupled in any fashion with that of Robert Brown. But you are a bold man, for I am sure that you will be shocked at if not a few botanists. I have never been so honoured before, & I hope it will do me good & make me try to be as careful as possible; & good Heavens how difficult accuracy is. I feel a very proud man, but I hope this will last. —— I needed to read your articles in the Nation & G. Chronicle, & my interest by them to me; but I could not conceive, (as I read them first in the G. Ch.) who could have written them. What you tell me about the trail of insects in Australia & the seeds on Orchids! I am now hard at work getting my book on Drosera ready for printers, but it will take some time for I am always finding out new points to observe. I think you will be interested by my mention in the digestion pamphlet in Drosera; the secretion of the acetic series contains an acid & some ferment

Down,
Beckenham, Kent.

I found at Aden that Felis & Bos inhabited Madagascar, I thought it was a fabulous story, & did not perceive it was a body like I saw to be women. — I had heard before of the wolf story, & knew not what to think of these scientific statements.

When you have any communication with Dr Rand, will you thank him much for the Notes of Ecos: I have been

glad to see the account,
but it is too late
for use, as I have
finished correcting the
early sheets for a new
Edit. of the Descent. I
have been forced to say
that I do not feel so
confident about the
"Darwinsche Anschauungen" as
the German edition, and I was before. —

Give my kindest
remembrances to Mrs
Gray. My wife & self
have so often played
backgammon every evening &
I often think of the
games between you & Mrs
Gray. — My wife thanks
you sincerely if I trespass
too much —

My dear Gray

Yours gratefully & sincerely

Ch. Darwin

104

June 5

Down,
Beckenham, Kent.

My dear Gray

I have now read your article in Nature, & the 2 last paragraphs were not included in the slip sent before. I wrote yesterday & cannot remember exactly what I said, & how cannot be easily without again telling you how profoundly I have been gratified. Everyone, I suppose occasionally thinks that he has worked in vain, & then on, there falls overall like me, I will think of you

article, & if that does not
appeal to evil spirit, I
think know that I am at
times a little bit insane,
as we all are occasionally.
What you say about Teleology
pleases me especially, & I
do not think anyone else
has ever noticed it point.
I have always said you were
the man to hit the nail
on the head.
Yours gratefully & affectionately
Ch. Darwin

108

Down,
Beckenham, Kent.

June 25th 74

My dear Gray

I said in my last note that I was at work on Drosera. Well it proves an excellent digester of fibrin, albumin, meat, &c. It has also interested me to find that if a row of flies be placed near the margin, the edge of the leaf in the course of 2 or 3 hrs turns over, so as to bring the secreting and absorbing glands into contact with the upper as well as the lower

surface of the flies. But the point which has interested me most is that the leaves certainly absorb nutritious matter from little leaves & seeds which are blown onto them. Hence the plant is certainly not only insectivorous, but graminivorous & granivorous! I have had a lot of leaves sent me from N. Wales; & it is extremely rare to find a single leaf without more than one captured insect. ~~They~~ Each had also more than one leaf, on an average, ± of some other plant adhering to them; and two seed-capsules

But it is rather too early in the year I suppose for seeds. Now does Pinguicula grow anywhere near you? & if so would you look to a certain number of leaves, & tell me whether you find any seeds (& knots) or leaves of other plants adhering to them. This would be valuable aid. It will amuse me much to make a good case about the omnivorous habits of this plant

Yours very sincerely

Ch. Darwin

109

Down,
Beckenham, Kent.

June 30

My dear Gray

I have received your two letters & have forwarded the enclosure, with the page inserted, to Nature; & I hope the Ed will publish it. It was very good of you to send me the Sarracenia; I soaked it & then stuffed it with cotton wool, so that I have now an excellent idea of the plant. It is as wonderful a case as any orchid Drosera or Dionæa

& I cannot say more in honour of a plant; I see that the lid is brightly coloured, no doubt to attract insects.

I read with interest your semi-theolog=ical review, & have got the book, but I think your review will satisfy me. The more I reflect on the subject, the more perplexed I grow.

I wrote a week since to ask you to observe if possible Drosera. I forget whether I mentioned that the margins & the leaves have the power of movement when excited by solid objects such as bits of glass or by nutritious fluids, but best of all by the two stimulants combined. I believe the purpose of this movement (but I am not yet sure) is to push flies further on to the leaf, when washed by rain into the narrow channel formed by the naturally involuted edge.

Yours most sincerely

Ch. Darwin

P.S. I do not think the I wrote a bit too strongly about your article on me...

I will of course send you a copy.

The death of Mr Hooker has indeed been a terrible blow. Poor Hooker came here directly after the funeral & took up manfully. I know I would much sooner die than suffer such a loss

My dear Gray

Yours affectionately

Ch. Darwin

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

Dec 25·74

My dear Gray

Many thanks for your kind note of Dec 11. I received & read with very great interest your article on the longevity or duration of varieties. This is a subject on which I have long felt interest; tho' I never before, as far as I can remember connected it with inter-crossing. I think you have put

the case very well & clearly. I heard lately from Mrs Treat about Ultrcularia; but she does not go very deep into any subject, & I have very great difficulty in believing some of her state=
=ments. I have got the whole of my book in M.S, but I do not know how long it will take me to get it ready for the printers; I hope it will be out late in the spring &

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Aug 30th

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

Would you be so kind as to get & send me the seeds of a packet of seeds of Nepeta verticillata. I want to raise seedlings for illustrations, mainly to see if it seedlings are sterile like hybrids & like to illustrate offspring of hybrids; for to fact seems to me all important. —

Ever yours
C. Darwin

not seen a scientific social for a very long time. Hooker seems to be absorbed in all sorts of routine work, and I fancy that you suffer largely in the same way. —

Believe me my dear Gray
Yrs ever sincerely
Ch. Darwin

Pray give my kind remembrances to Mrs. Gray. I know that the little I hear men boasting, — it refreshes them so much. Now the tally with my wife in back-gammon stands thus: she, poor creature, has won only 2490 games, whilst I have won, hurrah, hurrah,

2795

games. —

111

DOWN,
BECKENHAM, KENT.
~~RAILWAY STATION~~
~~ORPINGTON S.E.R.~~

Jan 28. 76

My dear Gray

I have to thank you for a whole pile of things, but beyond every thing else for the reviews in the Nation & Journal of Science. I do not think it would be possible to have given a fuller & clearer account of all my work. Although my own book is of course well-known to me, I found your articles quite instructive, and as you may well believe very pleasant to my vanity.

I was much interested by your little essay on the diversified means of the dispersal of seeds. Do you know Hildebrand's

"Verbreitung's mittel"? It is a capital essay; and he gives a great many analogous cases, but none more striking than yours. Lastly many thanks for your letter with the facts about Maurandia: what would I not have given for them when I was preparing the new Edit; but it is now too late, for I do not suppose I shall ever again touch the book. After much doubt I have resolved to act in this way with all my books for the future; that is to correct them once and never touch them again, so as to use the small quantity of work left in me for new matter. By the way there was an article in the last Gardener's Chronicle well worth reading on vine tendrils. I am now getting ready a book on the advantages of crossing, which will be a sort of complement to my orchid book, as this was devoted to the means of crossing. I have given you a long tirade about myself, but I have nothing else to say as I have

Aug. 9th/76

DOWN,
BECKENHAM, KENT.
(RAILWAY STATION
ORPINGTON. S.E.R.)

My dear Gray

I have just received "Darwinian" & am much obliged for it. I am uncommonly glad that you have been urged to take this step, not only on my own account, but for the public good; for every one of your articles seemed to me excellent. I will soon read to others, but I think not to attempt to resist reading

they are published. I suppose that you are working away as hard as ever. I think the Aden one gets the more there is to do. Pray give my kindest remembrances to Mrs Gray.

Ever yours very sincerely
Ch. Darwin

Did you have time to glance at my cousin Francis' paper on Stipules: he makes out nicely that the sensitivity depends on the lowness of each separate cell. He has also made a fine discovery, but it is too long a story.

The two new articles finish
I see by Table of contents
that you dip into the subject,
viz. the meaning of sex
on which I have written
in a new book now
going to Press — "The effects
of crossing & self-fertilisation
in the vegetable Kingdom".
This will complete all
that I think ever to do on
this subject. I am, however,
preparing a new edition of my
Orchid book, & this has led
me to several doubts of
your about notices on this
subject. But then they
been so much published,
that I have been afraid of
to give the briefest
abstract with references
of what has been done. As it
is I have had to cut
up the book unmercifully.
Of course both these books
will be sent you when

113

Oct. 28th 76

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

I sent by this post all the clean sheets we got printed, & I hope to send the remainder within a fortnight. Please observe that the first chapters are not readable, & that ~~about~~ the last very dull. Still I believe that the results are valuable. If you review the book, I shall be very curious to see what you think of it,

for I can more for you
judgment than for that of
almost anyone else.
I know also that you will
speak the truth, whether
for approval or disapproval.
Very few will take the
trouble to read the book,
& I do not expect
you to read the whole,
but I hope you will read
latter chapters. — Mr H.
Appleton will bring it
out in America, & my orchid book,
which I am now
correcting the sheets of,
as it has been almost
remodelled. —

Yours very sincerely
Ch. Darwin

I am so sick of correcting
the proofs & licking my horrid
bad style into intelligible
English.

4'

I send by this post sixty
sheets, some not proofed,
but corrected. Title &
Index shall be sent
hereafter. —

C. Darwin

Dear.

# FOREIGN POST CARD

FOR COUNTRIES INCLUDED IN THE POSTAL UNION.

THE ADDRESS ONLY TO BE WRITTEN ON THIS SIDE.


Prof. Asa Gray
Cambridge
Boston
Mass:
U. States

114

DOWN,
BECKENHAM, KENT.
~~RAILWAY STATION~~
~~ORPINGTON. S.E.R.~~

Nov 27. 76.

My dear Gray,

Many thanks for your pleasant letter, & for the correction of my stupid blunder. I hope that you will have received by this time ~~the sheets~~ a nearly complete set of sheets; but in case they should have failed, I send another set with the exception of the four or five last pages & title page

which will come very soon.
I shall be delighted to send you
sheets in advance of the Orchid
book, & have written for them today
Only yesterday I discovered that
I had over-looked one of your
papers on Platanthera, from which
I could have extracted two or three
good facts, & this has vexed
me. I find it very difficult
to keep references distinct for
various subject.

Ever yours very sincerely

Charles Darwin

115

DOWN,
BECKENHAM, KENT
RAILWAY STATION
ORPINGTON. S.E.R.

Dec 4. 1876

My dear Gray,

I think I told you that I was going to re-publish my Dimorphic papers, with additions. I have become convinced that plants of this class cannot be recognized merely by the varying lengths of pistils & stamens in a few specimens. It is necessary to compare size of pollen grains & state of stigma. Therefore I want you much to send

Rhamnus.

me one or two flowers of both forms of Leucosmia & Dry= = mispermum, (mentioned in Amer: Journ Science Vol 39 p 104 –) if not very rare & precious. The flowers ought to be rather youngish ones otherwise the pollen will have been shed or lost. My object is to see plants in as many natural families as possible, and if you could spare me flowers of any dimorphic plant, not included in the Primulaceæ, Lineæ Oxalideæ, Gentianeæ, Verbenaceæ, Boragineæ, Rubiaceæ & Lythraceæ, I should be very much obliged.

One other question, do any of the dimorphic plants known to you inhabit water or marshes?

I hope & suppose that you have assistants whom you could direct to look out for the desired specimens.

My dear Gray
Yours ever sincerely

Charles Darwin

116

Dec 20. 1876

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray,

Very many thanks about Hottonia. You mention Fra=xythia in the American Natural=ist, and I have just examined dried flowers from Kew, & find that F. suspensa is beautifully di=morphic; so I have got a new family. I have been thinking about your proposed new terms, and I cannot for very shame change again

I have used this term in two or three printed articles, and it is used by several German & Italian writers. Kuhn objected to the term on the same ground as you do; but no one objects to Vertebrata, ~~with~~ because it includes ~~with~~ an animal without vertebræ. Moreover heterostyled seems to me more definite than heterogone, as the latter would apply to di & monœcious & to polygamous plants. I am of course not able to appreciate the difficulty of working in the term in systematic works; but Thwaites speaks of <u>forma stylosa</u>, & why may not a species be called <u>heterostylosa</u>? However this may be it really would be too ridiculous for me to change again, so I remain

Your affectionate & obstinate friend

Charles Darwin

118

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

Jan 3. 1877

My dear Gray,

I have just received your letter of Dec 22nd. It is very good of you to be willing to send me such rare specimens; but I could not in any case think of accepting your offer on the more chance of making out whether the plants are heterostyled. The chance moreover would be very small without better spec=imens, as with dead plants

P.S. I send the Rhamnus branches with more opinions, but not decisively; if it is hetero it belongs to the with a rare Rhamnus to explaining the Rh Cathartica of

no evidence I think is sufficient except difference in size of pollen grains. I have lately given Kew so much trouble that I have vowed I would give no more for some time; but hereafter I will find out whether they can spare me any of the plants in question.

I am very glad of the notice about the black pigs: my faith in Wyman is so great that I have not been shaken by Wallace, who founds his speculation on a very feeble basis.

With hearty thanks
Yours very sincerely

Ch. Darwin

P.S.
Whenever you write again, can you tell me whether Primula Mistassi=nica, Linum virginianum & Bootii are dimorphic?

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DOWN,
BECKENHAM, KENT
RAILWAY STATION
ORPINGTON. S.E.R.

Jan 23. 77

My dear Gray,

Thanks for the card about Ponlederia, & I have written to Mr Leggett. I am going to trouble you once again but I honestly believe for the last time. In a letter dated Nov 21, 1870 you say that Phlox subulata presents two forms which have been named as species & which you are inclined to think is a

case of di or tri morphism: you speak of this as a common species, & if so could you send me two or three dried flowers of the different forms that I might compare their pollen grains & stigmas. In the same letter you mention Gilia aggregata (pulchella) as ~~differing much in the length of~~ ~~the~~ with the stamens & pistils varying much in length; I suppose this is a rare plant; but if not so, I should much like to examine the two forms. —

Forgive me & believe me

Yours very sincerely

Ch. Darwin

In Crosse Fertilisation

Pray correct a misprint

erratum

p. 275, 6 lines from top

for self-fertilised plants

read cross-fertilised plants.

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Feb. 18th 1871

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

From abstract of my book is

wonderfully good. You have

given everything, — you

have quite vivisected it.

By Jove I ought to

owe you a grudge!

I cannot it could not in

my opinion be improved.

Very many thanks, also, for

the specimens, & for not

hating me for bothering

you so much. In about a week's time I think my anima[illegible] will be greatest interest. I think send my son Frank to Kew on Wednesday to look at specimens of Leucosmia & some of the Plumbaginaceae &c.

I hope to get this off my hands & to think publish in June to profit by coming seeds of Gilia. —

I did despatch Title &c of Orchis book, but have now written to Murray to send you by post complete copy.

Some of your criticisms & suggestions in your two reviews are very good.

Ever yours gratefully

Ch. Darwin

over

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March 8: 77

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

Perhaps you will like to hear what little I have been able to make out about your flowers.

Lencosmia Burnettiana is in all probability dimorphic giving the relative length & positions of stamens & pistils, but more especially the difference in the stigmas of the 2 forms, but the pollen grains do not differ in size, which is the best evidence. —

at an without feeling prepared pity for all botanists, but I suppose you are used to it like eels to be skinned alive. —

With hearty thanks

Your friend

Ch. Darwin

Gilia pulchella: the two forms differ in their stigmas & do not differ in their pollen grains; & I think has left this case quite doubtful, had not Gilia Nivea the differed in exactly the same manner in the stigmas & moreover in the diameter of the pollen-grains: therefore I do not doubt this both Gilias & in which you allude they are truly heterostyled. others &c are

Phlox subulata is a dimorphic in character & as bad as Rhamnus: perhaps it was once heterostyled, with the shortstyled form since ended more feminine in nature.

Altogether I now know of fairly good evidence of 39 genera, in 14 families, which include heterostyled species. This pleases me.

It is dreadful work making out anything about dried flowers; I never look

I am now trying to make out the use or function of "bloom" on the waxy secretion on the leaves & fruit of plants, but am very doubtful whether I shall succeed. — Can you give me any light? Are such plants commoner in warm climates than in colder climates? I ask because I often walk out in heavy rain & very few wild dicotyledons can be seen with drops of water

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June 4th

DOWN,
BECKENHAM, KENT
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

Prof. Babey's case has come too late for me, as to sheets on this subject are printed off. He indeed if it had come earlier, I should I have known what to do with it. The pollen-grains & stigmas ought to be compared. He can seem to be well worth careful investigation & I wd. have given my eyes for seeds formerly; but now I have done with the subject.

If Prof. Babey likes to experiment walk he might raise seedlings & ~~soft~~ fertilize them & bring ~~together~~ pistils

with pollen from big & short stamens from distinct plants & on the same plant: counting the proportion of flowers which set fruit, when fertilised in the various ways & the number of seeds per fruit. This diagram shows the nature & the difference between the flowers excellently. I will send him my book, when published in 4 or 5 weeks, & if he thinks it worth reading he can see how to experimentize on the plants. — The case may be one merely of great variability, but it may be one of incipient heterostylism & under this point of view I would formerly (if I could) have investigated it most carefully. —

When you receive my little book, you will see that I have drawn an analogous deduction with respect to you. —

rolling off the Thorn like quicksilver.
Where in my flower garden,
gladiolus & lobelias have
an hundred: again an
blown-protected plants common
on the western plains:
[Sir Joseph Hooker ~~of~~ on my dry
Hooker thinks that they are
common at the C. of Good
Hope.] It is a puzzle to
me if they are common
under my dry climates, for
I find blown very common
on the Acacias & Eucalypti
of Australia. Some of the
Eucalypti which do not

appear to be covered
with bloom. Has the epidermis
protected by a layer of
some substance which
is dissolved in boiling
alcohol. — Are there any
bloom-protected leaves or
fruit in the arctic
regions? — If you can illustrate
me, as you so often have
done, pray do so; but
otherwise do not bother
yourself by answering.

Yours affectly
C. Darwin

Jan. 21st 78

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

I received 2 or 3 days ago your review of my Forms of Flowers, which has pleased & as usual instructed me much. I was specially pleased at my suggestion of giving names to the subdivisions of polygamous plants being approved of by you. Thanks, also, for the review of Mr Cooke, written, I suppose by you. It seems to me rather too mild, but I have been very glad to read it, & have been amused by it. — By the way Carlyle's letter about me was a forgery, or as he said to my

but we very much want the use of Gloxinia or to wage secretion on the leaves & stems of plants.

Did you notice whether red glaucous plants are more or less common in the arid districts to the west or centre there in the more humid districts on the Atlantic?

You know my opinion of the American ladies, so you will believe how delighted we have been at my son marrying Sara Sedgwick — She is in every possible way quite charming.

Yours affectionately
Ch Darwin

Brother, — "it is all informal like" —



Could you not get some young man to experiment on & discover grades of fertility of Epipactis & Rhinanthus. Hermann Müller describes Valeriana dioica as consisting of 4 analogous forms. He attributes these case primarily to the existence of 2 forms, one with larger & the other with shorter corolla, such as he has shown exists in other cases; but his German was obscure, & I could not follow the further supposed transformations.

I hope this you have got over the first great pressure of a committed work & are now at a much more work. I hope tomorrow to see Hooker, for I am writing this in London, to which place I have come for a few days rest after working rather too hard. My son Frank & I have been observing the autumnal movements of seedlings & these due to Heliotropism, which latter fact this complexity have almost driven us mad. We have made out something,

almost tempted to write finally to the Phil. Soc. to inquire how it can really stands. But I have written to Mr Hilgard, as to Mr Meehan's inaccuracy seems to my mind to depend to Science

C. D.

That you may be a Conservative like a cat

I have just spent a delightful 2 hours at Kew, & heard prodigies of your strength & activity.



DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

P.S. I forgot to add the following as I wished to do yesterday. Mr Meehan in a paper lately read before the Philadelphia Soc. says in a comment showing how the plants behave differently in one country from another for the Linum perenne brought from Colorado, he gives quite fertile with him, where I thought (confirmed by

might have added of Hildebrand)
that it is absolutely identical
with the new species.
Now he does not tell us
whether his plants
were long-styled or
short-styled, & as he
speaks of bringing the
plants from Colorado,
I imagine that it
was then endemic.
Does Dr Greene grow
them? Dr Alefeld says
none of the
American species are
dimorphic. Now if Mr
Meehan has mistaken the
species it seems to me
too bad to throw a
slur on Gray or
another main accuracy
without taking the slightest
pains to be accurate
himself. I have been

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Feb. 17th

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

I thank you much for your pleasant letter of Feb. 3rd. Hooker has been here 2 or 3 times (as Lady H. has been here for a fortnight as a sanatorium) & I told him about Lincoln's presence & he was interested & looked to his Colorado specimens. He finds "the American form "is less strongly heterostyled than "the European, & that the stamens

"& Hybes are even equal
"in rare specimens; he
he also finds variability
in length in the European
specimens. —

If I was forced to
wager I wd bet
that the American
form would prove at
least functionally a distinct
species. — If you
could get & send me seed of
the Colorado form, I wd
give both forms a
fair trial if they could be
uninterrupted artificially, &
I would try whether
the homostyled
individuals were
self-fertile. —

Yours affectionately
Ch. Darwin

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Aug. 15th 78

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray.

M. Hugo de Vries, who has ~~been~~ such excellent work on climbing plants & I by suggestion wished to make him examine on the twitch of Echinocystis lobata (I think this is the right name, but I am writing away from home) of which you formerly sent me seeds. – He has tried sunflower & sat seeds & I have offered to

write to you. — If you
can I beg you to
send him such address
to Prof. Hugo de Vries
Amsterdam
He is Prof. of Botany there.

I see that we are
both elected Corr. members
of the Institute. It is rather
a good joke that I
shd be elected in the

Botanical section, as the
extent of my knowledge
is little more than that
a daisy is a compositous
plant & a pea a
leguminous one. —
Excuse your very tiresome
Ch. Darwin

I am very anxious for
De Vries to test some
of my observations on
Echinospitum, &

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Oct 24/79

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

I have procured & been reading your new Edit. of your Text-Book (which has been greatly developed since Ed times) of Botany, & I find at p. 21, 22, a curious account of Ivy seedlings. For a botanist specially of your being able to send me a few seeds of the 2 kinds which I want most, I have written their names down on the next page. — I have procured Delphinium nudicaule for a nurseryman. I have attended

attended I thought to the manner in which seedlings break through the ground, & it is for this object that I want these seeds.

I have written a rather big book, — more in the fifty on the movements of plants, & I am now just beginning to go over the M.S. for the second time, which is a horrid bore.

I hope that you & your are both quite well. —

Give my love to Emma

Ever yours affectly

Ch. Darwin

Dr Veiten has been working at the habits of insectivorous plants

seeds wanted

Ipomoea leptophylla

Megarrhiza californica

Dec 16 1879

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R

My dear Gray

It is uncommonly good of you to have taken so much trouble about the seeds; but it makes me feel rather guilty, for though I was very anxious to see them germinate, yet in question was the point of much importance. I found a leptosyphon has not yet germinated, but I have a good many seeds to sow again in the early

some of seeds of the (Leptosyphon), I ought to be glad; common cotton, for only enough I can get. The seeds in England are very — have written for more to abroad for Kew, but seem doubtful of the result. — The cotyledons behave oddly at night, for when old & very when old they sink downwards, & I did not when I had sufficiently investigate the point sufficiently.

Yours very truly

Ch. Darwin

spring, if there already grown do not germinate; & I have often suspected that some kinds of seeds have an obstinate inherited habit of not germinating & dying if sown in the winter. — I have just for 5 of the seeds of Megarrhiza to Joseph, but of one sticks & thus alarms me. — I rg. would wait to see whether the curious heel-like projection at the base of the hypocotyledonous stem, described by Flahault, & which splits the seed coats so beautifully in the Cucurbitaceæ, is here absent, & as I hope & as ought to be the case as the Cotyledons are not withdrawn from the seed-coats.

Now do not return your rg. valuable time, but if you c^d easily send me

the radicle is coloured brown whilst the hypocotyl & cotyledons is left uncoloured. Now when a seedling Megarrhiza with the plum=ule just reaching the surface was thus treated, the whole radicle (+ hypocotyl) & the whole of the tubular petioles (densely covered with root hairs) became brown whilst the plumule was quite uncoloured. Therefore I think it certain that the tubular petioles act functionally like a root and that the cotyledons are hypo gæan. The sole use of this

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

Jan 19. 80

My dear Gray,

I have been greatly interested with the Megarrhiza seeds which you so kindly sent me. You have been mis=informed about their germin=ation, for I think you cannot have watched the whole process. Some were placed by me on, and others half an inch beneath the surface, and others deeper,

but none of the cotyledons were lifted up. One seed on the surface was a little tilted by the root not penetrating the ground, but this often occurs with all kinds of seeds. The petioles of my specimens were not stiff enough to bear the weight of the seed. What takes place is that the radicle bends down & penetrates the ground, but grows only to a length of about half an inch or less (length rather doubtful as I did not wish to kill specimens by making sections). When of this length its growth is arrested, and the lower ends of the tubular petioles grow quickly & penetrate the ground just like a root to a depth of nearly $2\frac{1}{2}$ inches; then their growth ceases, and now the radicle takes up the game & grows very quickly. In every case the base of the radicle lay ~~too~~ $2\frac{1}{2}$ inches beneath the surface. You probably know that if ordinary seedlings are placed in solution of permanganate of potassium

2



DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

wonderful manner of growth which occurs to me is to hide the enlarged root, at least at first, beneath 2½ inches of soil as a protection against enemies. When my plants are two or three weeks old I will cut a slice from the root, and taste it & test it for starch. Now can you tell me whether the plant is an annual or perennial? When the root has become huge does it come to the surface, and is

Is them hard, ~~or~~ and is it then bitter?

I wonder whether it is attacked by beasts birds insects or slugs in California?

It is has been a great grief to me that not one of the seeds of *Ipomoea leptophylla* has germinated: my gardener opened some & found them rotten. I hope I haven't wearied you much

Ever, my dear Gray,
Yours sincerely

Charles Darwin

Jan 26

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Guy

This is a P.S. to yesterday's letter to say that the drawing at p. 21 of your text Book wo. represent perfectly all that I have seen, supposing that a line were drawn, representing the surface of the soil, 1/2 inch above the seed, assuming that the seed had been sown at 1/2 inch depth. — You may

perhaps like to know the
the first time leaves
break out through a
split at base of
confluent petals of
<u>Delphinium</u> <u>nudicaule</u>
precisely as in Megarrhiza.
The growing of the plumules
bursts the tube, & then
the bowing downwards of
the tip of the plumules of
Megarrhiza, forces it ~~out~~
laterally out of the tube.
The tip is at first straight.
This bowing down of the
tip is a very common a
tellers ~~movement~~ movement
with seedlings, but here
it plays a new part.

Yours
C. Darwin

I ~~take~~ from radicle, with a minute plumule.

Ever yr dear Gray

from yr sincerely

C. Darwin

P.S. Some of the seeds received look rather a little flattened & evidently different; they were sown separately, but not one germinated.

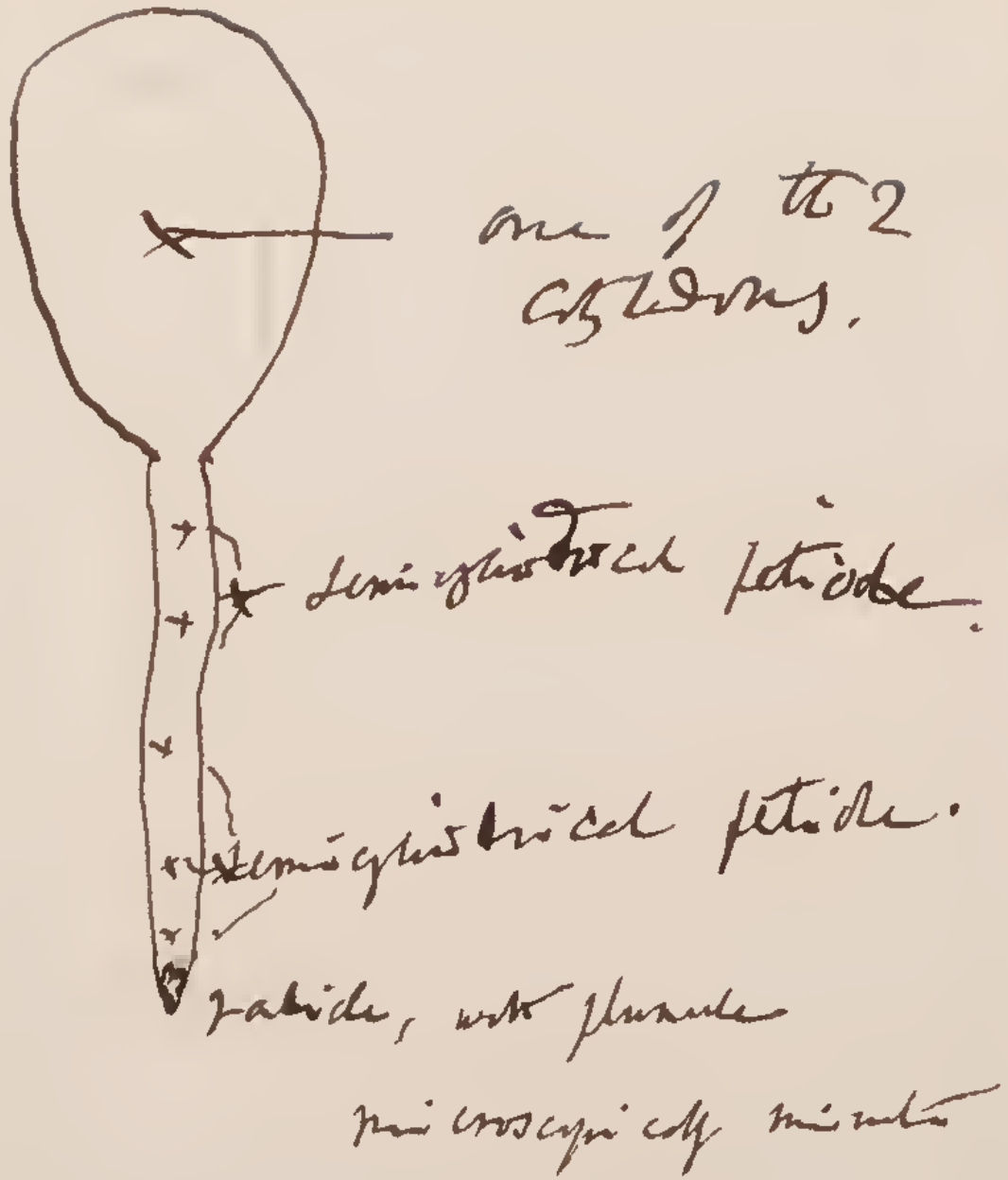
DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

Feb 17/80

My dear Gray,

If ~~your~~ my letter opened your eyes, yours has opened mine much wider. It is very strange that plants, if they belong to the same species, should behave so differently. The seeds were laid on the surface or buried in a mixture of peat sand & common soil, & this may have yielded more easily than

your soil. From the extraordinary intermission in the growth of the true radicle, from the root-hairs & from the petiole staining brown with permanganate of potash I must believe that the normal function is to bury itself. What ~~a strange fact it is that in my~~ case the petioles ~~in my case~~ were positively ~~heliotropic~~ geotropic & in yours ~~were negatively so~~. My plants are growing very vigorously. Should they flower, I will send you dried with leaves, for the chance of your being able to name them. — I am astonished at the whole case. — I suppose when the petioles grow in the air they are stiffer than when by so soon, for mine could not support the weight of the cotyledons. One had germinated abnormally; the cotyledons emitted the petiole, which was a hollow 1/2 cylinder, as in sketch with new enlargement


one of the 2
cotyledons.
semicylindrical petiole.
semicylindrical petiole.
petiole, with plumule
microscopically minute

forgot to state that Dr Krause had altered his article on Erasmus Darwin before sending it to England for translation.

Ever yours sincerely

C. Darwin

March 24th

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

I thank you much for the 2 sets of Megarrhiza: I hope that they may germinate for I should very much like to see a longitudinal section & the proportion of the parts, when the (apparent) root is of 1/2 or 1/3 of which not of the seed-coats.

You must not suppose that what is obvious to you is so to me; for as the comparent petals of the Cots. of the Delphinium are not tubular at first, I was

astonished to see the young leaves coming out of a hole or slit at their base.

Very many thanks for all your information about the gorthigern, the germination of which has interested me greatly.

I was much amused by your little article on the Philadelphia lawyers. You are a first-rate hand in touching up a fool. The lawyer is a cool man in trying to make an out a rogue; but this seems to fashion according to Mr S. Butler in the Athenæum I am a rogue of the deepest dye, because I

Jan. 29th 1881

DOWN,
BECKENHAM, KENT.
RAILWAY STATION
ORPINGTON. S.E.R.

My dear Gray

It was extremely kind of you to send me your review on the journal of that in the Nation. — It has pleased me greatly; for there is hardly any one in the world whose approbation I value more highly than I do yours. That was a stupid blunder about Apium; but you cannot put yourself in my frame of mind: "Celery" calls up an image

letter for you some little time ago, & he did not answer it, having nothing definite to say, simply from not liking to trouble you. —

I hope when you come back from the continent that you & Mrs Gray may find time to pay us a little visit. —

Once again thanking you for your kindness, & trusting to see you for my many sins I remain, Dear Sir, Sincerely

Chas Darwin

in my mind, but not the word opium. — As for Aspic I remember the name, & I fancy in connection with climbing plants. I am a pretty sort of man to write about plants!!

Nothing in your Review pleased me more than your saying something about Frank. If you knew him well, you would know that such an idea as being offended with you never could crop in his mind. In fact I wish I could infuse a few drachms of vanity & self-conceit into his veins, for he never with ~~thinks~~ value in the least what he does. — Therefore I am certain that the notion of wishing this you would speak in his praise wd. never have occurred to him. — He was pleased by receiving a